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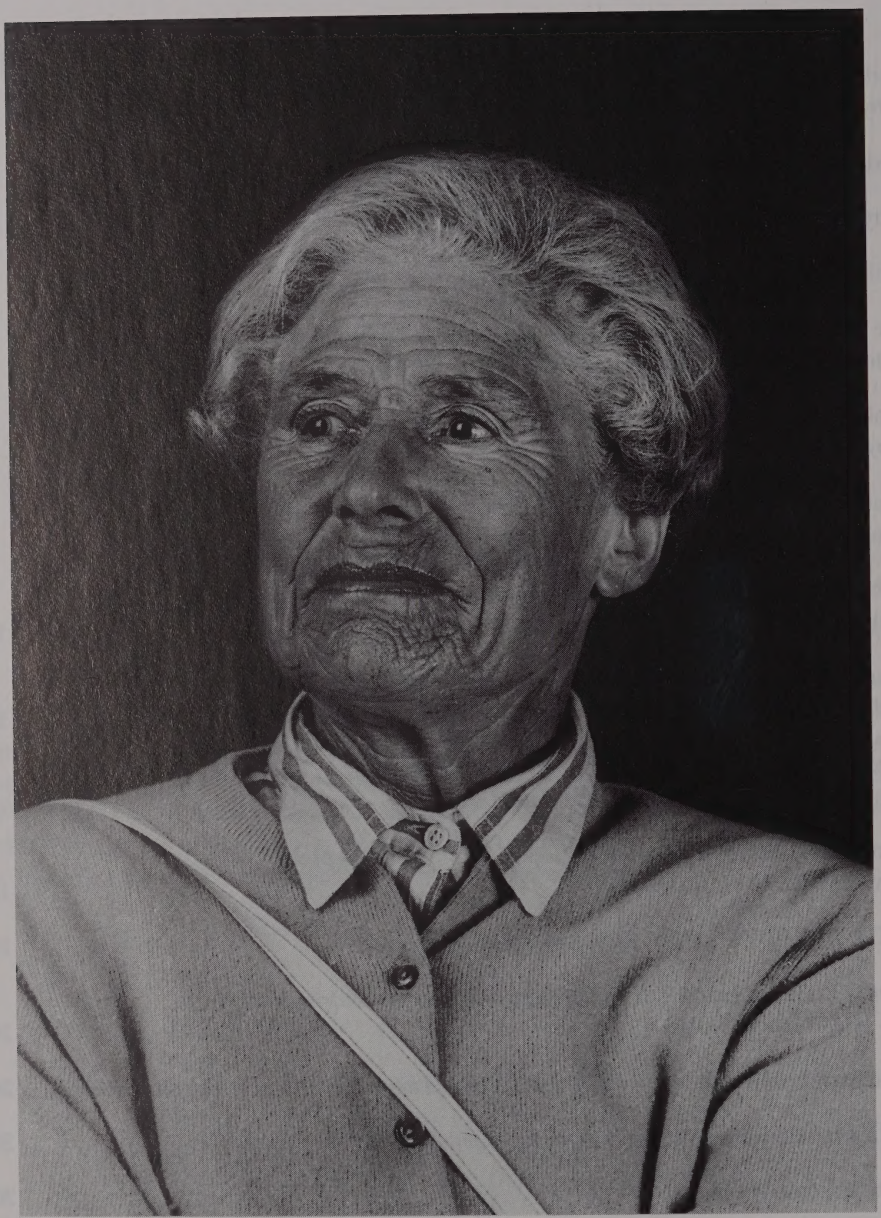

Barbara Parker Mallowan and the Institute of Archaeology

by RACHEL MAXWELL-HYSLOP*

Barbara's connection with the Institute began in 1935 when she was one of the original three students, accepted when it existed only on paper, before the official opening at St John's Lodge on 29 April 1937, by the Chancellor of the University. It was not until 1961 when she returned from Baghdad, after 12 years of distinguished service to the British School of Archaeology, that she joined the staff of the Western Asiatic Department as Lecturer in Western Asiatic Archaeology under Sir Max Mallowan. She certainly enjoyed teaching, especially foreign students, and anyone with particular difficulties of language or expression profited from her conscientious and civilized approach to Near Eastern studies. Teaching, combined with an enthusiasm for research, resulted in a series of articles published in *Iraq*. Dominique Collon has written about her contribution to the study of cylinder seals (see below); an account of her work on the tablets from Nimrud and Balawat must also be given to illustrate the breadth of her approach to problems connected with both linguistic and archaeological evidence.

Especial emphasis was often given to her students about the importance of the use and understanding of written evidence afforded by cuneiform tablets alongside that yielded by excavation reports recording architectural remains and stratified artefacts. This was perhaps due to the teaching of Professor Sidney Smith in the extraordinary postgraduate course of Mesopotamian archaeology provided by the Institute at its foundation and which now no longer exists. It is interesting to recall its requirements. The seven papers (with no choice allowed) consisted of two for Akkadian language, one Ancient History, one History of Civilisation, one Religion, one Laws, Letters and Contracts (with questions written in cuneiform and transliterated Akkadian) and finally one entitled Art, Architecture and Technical Processes (this included pottery). Barbara's success in attaining the high standard demanded by the examiners in this rather formidable and eclectic course of study formed the basis for her teaching of both Mesopotamian religion and cylinder seals at the Institute and also enabled her to continue linguistic studies in order to undertake the publication of the *Nimrud Business Documents* in 1954, *Economic and Legal Texts* in 1957, *Administrative Tablets* in 1961 and *Economic texts from Balawat* in 1963.

Her understanding of the activities of every kind of inhabitant of Assyria in the eighth century BC, especially the detailed records of the bureaucratic palace officials, may have been due, as she often remarked, to her own experience in the Civil Service at the end of the

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last war, in the (then) Ministry of Works and Public Buildings. This also entailed composing answers to Parliamentary questions that certainly could never have been considered an 'art form'. But the intricacies of Assyrian civil and economic administration, with the diverse activities of a great variety of individuals such as shepherds, reapers, diggers of irrigation ditches, servants of the queen's cooks, owners or grandsons of slaves, scribes or landowners with their disputes, agreements and problems recorded in witnessed contracts, can all be found in these articles. One often feels that Barbara herself, who was always concerned with the diverse activities of friends and colleagues, would somehow be completely at home in this milieu. Furthermore, the amount of information (metallurgical, botanical, geographical etc.), which can be gained from the lists of supplies and tribute lists sent to the Royal palace not only for the king but for his enormous entourage of diviners, charioteers, leatherworkers, messengers, exorcists and officials and craftsmen of all kinds, suggests that social anthropologists, prehistorians and field archaeologists working in areas outside Mesopotamia could well benefit from a perusal of at least the introductions to the translated texts.

Two other articles deserve attention. One published in *Studies in Honour of Nimet Özgüç* on 'Assyrian Temple Furniture' combines evidence from cylinder seals, existing stone altars and incense burners from Khorsabad and Nimrud with ritual and prayers concerning incense, and reference to the description of Shalmaneser III's banquet when he arrived at the source of the Tigris. The second, on 'Magic and Ritual in the North-West Palace Reliefs' again combines evidence from religious texts with archaeology. Her last contribution (jointly with the writer) appeared in May 1994 entitled 'A Stamp cylinder seal from central Asia in the British Museum' (*Festschrift für Ruth Mayer-Opificius*, Ugarit-Verlag-Münster).

Barbara will be sadly missed by her many friends and colleagues and, in an age of increasing specialization, her particular qualities will, one hopes, always be remembered.

Barbara Parker Mallowan and Cylinder Seals

by DOMINIQUE COLLON*

When Barbara returned from the Lachish excavations, Sidney Smith told her to write about the cylinder seals of Palestine (R. Maxwell-Hyslop, personal communication). The result was a lengthy article which appeared in 1949 in the journal *Iraq* and which is still the easiest reference to the cylinder seals from Palestine—both excavated and unprovenanced; some of the seals have never been published elsewhere. Barbara had become—and for decades remained—the only British scholar working on seals and one of the very few worldwide.

Barbara then became involved in the new excavations at Nimrud and took charge of the publication of the seals and impressions. These appeared in two instalments covering the periods 1949–53 and 1955–8, published in *Iraq* in 1955 and 1962. She then moved on to the publication of the material from Tell el Rimah: the seals in the volume of *Iraq* for 1975 and the impressions in those for 1974 (one impression) and 1977.

In addition Barbara wrote many shorter contributions on the subject of seals, but it is for the articles referred to above that she will chiefly be remembered. These were always clearly laid out, with all the relevant information, a useful discussion and good illustrations. They are immensely valuable.

Barbara also taught what she knew about seals to several generations of students at the Institute of Archaeology. I was one of them and she fired my interest in the subject. Barbara had been asked to write the catalogue of Old Babylonian seals at the British Museum; with characteristic generosity she arranged for me to write it instead. She also steered Lamia al-Gailani Werr through her Ph.D. on Old Babylonian regional styles; it has since been published and is the key reference on the subject. Lamia was Barbara's last student.

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Why study ancient America?†

by WARWICK BRAY*

In 1552 Francisco López de Gómara (secretary to Hernando Cortés, the conqueror of Mexico) published a *History of the Indies*. On the first page, in his dedication to the Emperor Charles V, he wrote these words:

> The Discovery of the Indies, what we call the New World, is, excepting the Incarnation and Death of Him who created it, the most important event since the creation of the world. (López de Gómara, 1965: 5)

What the Spanish had discovered was, of course, Native America, and with it a series of cultures that had evolved *in situ* over thousands of years without contacts with Europe or Asia.

Exactly five hundred years after this 'most important event' the University has conferred on me the title of Professor of Latin American Archaeology, the first there has ever been in a British university. Some might think that five centuries of indecision is carrying academic caution a bit too far, but I assure you I am appreciative of the honour done to me personally, and (much more important in the long run) I also take it as a vote of confidence in the subject I now profess.

Inaugural lectures[1] are a special kind of ritual occasion, with their own conventions, in-jokes, public and private agendas (Tilley, 1989). By tradition, a new professor makes generous reference to his predecessors, then describes his own interests and, finally, looks towards the future. I shall try to do all of those things, but I want to begin by making some comments about the past history of the discipline in Britain.

What is surprising is that there are so few predecessors to thank. The great names in academic archaeology when I was a student (Wheeler, Childe, Clark, Piggott and their contemporaries) had an impressively broad knowledge of Old World prehistory, but they ignored the Americas. Childe (1942: 21) clearly believed that aboriginal America contributed nothing to the mainstream of human history. Even in his theoretical works he hardly ever made use of American data (Flannery, 1994), and when he did (e.g. in his classic paper in *The Town Planning Review*: Childe, 1950) his comments were unperceptive, and the whole subject seems to have embarrassed him.

The only previous university teacher in my subject, and this long before my time, was the eccentric Thomas Gann, a one-time medical officer in British Honduras, teller of tall stories, and excavator of a remarkable number of Maya ruins (Carmichael, 1973). One of his most

† Based on Inaugural Lecture, given 2 June 1993.

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famous discoveries was the painted fresco at the site of Santa Rita. Gann's copy of this is, in fact, the only record we have of it, for it was ground up and eaten as medicine by the local people shortly afterwards. How it came about I have no idea, but from 1919 to 1938 Gann was Lecturer in Central American Archaeology at Liverpool University, the first Americanist ever to hold a university position in Britain. I have never come across anyone who went to his lectures (I am not even sure that he gave any) and he seems to have trained no students. Not surprisingly, the lectureship died with him.

It was freelance scholars and the museum world, not the universities, that kept American archaeology alive in the United Kingdom (Bray and Glover, 1987: 117–19), and it was fuddling around in museums as a small boy that first gave me a taste for exotic cultures, though with no particular preference for the New World. After a false start in English Literature at Cambridge, and having been turned down (with good reason) by both Egyptology and Mesopotamian Archaeology, I eventually found a home in European Prehistory. In the Cambridge of the 1950s nobody was expected to become an Americanist and there was no provision for learrning the trade. My only formal training in the subject consists of half a dozen lectures from the late Geoffrey Bushnell, a fine scholar and a good friend, whose official position was Curator of the Museum of Archaeology and Anthropology. For the rest, like the few other British Americanists of my generation, I am self-taught. I am not sure what our many and various masters would make of this today, but the experience has left me with a conviction that the best academic system is one that lets people get on with the job and does not place too much value on bureaucratic tidiness. Auditing people into uniformity is not the best way to produce creative research or exciting teaching.

But, putting present-day grumbles to one side, and returning to easier, if laxer, times, the next landmark in my American career (after a few years of respectability as a European prehistorian) came with my appointment by this University as Lecturer in Latin American Archaeology. The day was April Fool's Day 1967, and the appointment was a joint one between the Institute of Archaeology and the Institute of Latin American Studies. This, as I have mentioned, was the first full-time teaching post in the subject anywhere in Britain since the demise of Thomas Gann, and it is still the only one,[2] though there are specialists in pre-Columbian art history at both Essex and East Anglia.

I will argue later that this is an unsatisfactory situation, but it is time now to come back to the main theme of my lecture. When I mentioned to one of my students that I had chosen the title 'Why study ancient America?', her instant reaction was 'Well, why not?' The more I think about it, the more this seems a reasonable answer. I believe that if something is inherently interesting it is therefore worth doing. This is not the kind of justification that appeals to politicians or accountants; I suspect they would phrase the question quite differently, and ask 'Why should our taxes pay for anyone to study long-dead American Indians and their artefacts?'

I shall address both points of view by attempting to show that ancient America is interesting in its own right, and also has significance in the world outside academia. The text for my sermon is a clause from the 1992 revision of the *European Convention on the*

Protection of the Archaeological Heritage. The document emphasizes the value of this heritage as 'a source of the European collective memory and as an instrument for historical and scientific study'. These words are equally valid for America. It is precisely this 'collective memory' of the prehispanic past that makes American archaeology relevant to the world of today.

We can begin the analysis by returning to the theme of the Discovery of the Indies, 'the most important event since the creation of the world'. Many of us have mixed feelings about this, and especially about the ways in which the event has been packaged for public consumption five hundred years later.

Last year, in 1992, 32 nations celebrated the Quincentennial of the Columbus voyages, with parades, pageants, firework displays, conferences, exhibitions, monuments, innumerable books and television programmes, commemorative coins and stamps, sailing races, and still more bizarre forms of observance (Sale, 1990: 361). Altogether, 12 full-scale replicas of Columbus's ships were planned, though I'm not sure how many were actually built, and, anyhow, most of them sank. The ships commissioned by Spain alone cost about sixty times the amount paid for Columbus's entire first expedition. The Metropolitan Opera in New York commissioned a special work called *The Voyage*, and the Dominican Republic set out to build a lighthouse at a cost of ten million dollars. But my own award for lunacy goes to the private consortium in the United States which organized a 'marriage' between the Columbus Monument in Barcelona and the Statue of Liberty in New York, to be followed by a honeymoon trip and even 'twin-monument birth', with one offspring in each hemisphere. The logistics of this defeat me completely.

These kinds of goings-on, with their flagrant Hispanic triumphalism, are offensive to Native Americans and their sympathizers, but they also worry me for a different reason—they perpetuate the widespread belief that America was of no significance until it entered the orbit of *European* history in 1492.

Many years ago I asked the Navajo assistant in the bookshop at the Museum of the American Indian, New York, whether he had anything on the Discovery of America. He gave me a long hard look. 'Man,' he said, 'we were never lost.' And he was right; his ancestors had lived in what we presumptuously call America for at least 14,000 years before Europe had ever heard of it, and the true figure is probably around 50,000 years or more. There was, of course, no way that sixteenth-century Europeans could know about this, but, from the time of the Conquest onwards, the popular European view has regarded the native Americans as peoples without history.

There is a simple experiment that any of you can do. Walk into the nearest pub, say that you're an archaeologist who works in South America, and I guarantee that somebody will ask you what it's like to dig up an Inca city. If you're really unlucky, you'll be asked about dinosaurs too. Incas, Aztecs, human sacrifices—all too often these represent the sum of public awareness. The Aztecs and Incas have a high profile because they are the people that Europeans met and wrote about. As the most spectacular of the civilizations overthrown by the conquistadors, the Aztecs and Incas have been co-opted into European history—albeit on the losing side. But what of the previous ten thousand years?

I can't remember who first said that there were no Indians in America until the European conquest, but the remark encapsulates an important truth. The term 'Indian' is a European-imposed identity, designed to distinguish 'them' from 'us'. It also denies the ethnic and cultural diversity of the Native American world. The idea that the Americans had (and knew they had) historical and cultural traditions that go back as far as our own, is simply not part of the intellectual baggage of Europeans. One reason may be that, in most places and at most times, native America had no *written* history, and this is the only kind of history that really counts in European scholarship (Brotherston,1992).

Given the absence of long documentary histories (European style), and the difficulties of making European-style sense out of indigenous pictorial and oral records, the task of reconstructing the American past has fallen to the archaeologists. This, I have to say, is not the way many Native Americans see it. They already have their own, perfectly satisfying, explanations for the origins of the world, the emergence of humankind, the history of their own people and their place in the scheme of things. We dismiss these explanations by labelling them myths or legends. At the moment the two kinds of history seem as irreconcilable as the Book of Genesis with scientific geology, and it is hard to see how any synthesis can ever emerge. In the meantime, until the problem of 'multiple truths' is resolved, all the archaeologists can do is continue to write their own kind of history for their own kind of audience.

This is more than enough, for the archaeology of America's past is a huge field of study—approximately one fifth of the world's surface, from the decline of the Neanderthals to the rise of the Tudors. It is simply too big, and too important, to ignore. In a short presentation it is impossible to summarize all the achievements of prehispanic America, but I do want to show that, by any qualitative measure, the ancient American civilizations can be ranked with those of Mesopotamia, Egypt and the Classical world, or with any culture of pre-industrial Europe. If you accept this evidence, then it follows that any educated person should be as much aware of the Olmecs and Mayas, or the people who created the Moche and Tiwanaku cultures, as of, say, the Babylonians and the Etruscans. There are, in fact, signs that this awareness is coming at last; the Aztecs have been added to the official syllabus of British primary schools.

Native America has never produced a world religion, an international art style or a major item of industrial technology (hence, perhaps, Childe's dismissal) but it has made one enormous contribution to human well-being on a global scale. I refer to foodstuffs, crop plants (and the Christmas turkey) first brought into domestication in various parts of America thousands of years before the European arrival (Bray, 1993). The list includes maize, potatoes, manioc, kidney beans, squashes and pumpkins, the Jerusalem artichoke, tomato, pineapple, avocado, chili pepper, peanuts and cashews, cacao (chocolate) and tobacco. Of these, maize and potatoes come third and fourth (after wheat and rice) in the FAO figures for world production, and manioc (cassava) is not far behind.

Moving rapidly onwards in time, I would now like to make a more direct comparison between the Old World and the New. As Europeans we grow up with the knowledge that—

in technological, artistic, philosophical, moral and religious terms—we belong within a great tradition, that is often called Western Christendom. By definition, this tradition came into being about two thousand years ago, though its roots go back much further, to Rome, Greece and the Near East. American civilizations have comparable time-depth. From two thousand years ago we can recognize artefacts, technologies, deities, intellectual concepts and general 'ways of life' ancestral to those the conquistadors encountered in their 'New' World.

By AD 100 (equivalent to late Iron Age and Roman Britain) much of America was becoming civilized, no matter how one defines that difficult word. One regional example is all we have time for. At El Mirador, in the Guatemalan Petén, one of the largest Maya temple-pyramids, a stepped platform 70 m high, was built between 150 BC and AD 50. This was just one structure in an urban zone of 16 km^2. El Mirador is the largest of these early sites, but it is not unique. In fact, nearly all the principal lowland Maya sites were in existence by the time of Christ and, in more general terms, this was the period when Classic Maya civilization was beginning to take shape. It had all the characteristics we have come to expect of a civilized way of life: effective government, a state religion, fine architecture and art (saleroom prices for Maya pottery compare with those for quality Greek work), and, uniquely for pre-Columbian America, the Maya erected inscribed and dated monuments that we can now read, more or less. Glyphic inscriptions appear also on pottery, wall paintings, and in the form of screenfold books.

Inscriptions like these make it clear that the Maya were organized into city states, each ruled by a royal dynasty supported by a hierarchy of minor lords and officials. We have glyphs for the names of real people, for family relationships, and for the main events of a ruler's life: his birth, marriage, accession to the throne, wars and conquests, the taking of captives, state visits, and many different ritual activities. Sometimes we even have his bones. The discovery in 1952, by the Mexican archaeologist Alberto Ruz, of King Pacal's funerary crypt at Palenque was an event comparable with the finding of Tutankhamen or Phillip of Macedon. In the quality of their social organization and in their political behaviour I can see no great difference between the Maya city states and those of Classical Greece or Renaissance Italy—and nobody seems to doubt the value of studying those!

Turning to matters intellectual, in arithmetic and astronomy the Maya were, in many respects, as advanced as any nation of fifteenth-century Europe. Otto Neugebauer, a historian of science, lists place value numeration, the basis of all modern arithmetic, as one of the most fertile inventions of humanity. The Mesoamericans, who had independently invented writing, also invented an arithmetical system that incorporated place value and the concept of zero. This combination of writing and arithmetic allowed the Maya to calculate and express the things that astronomers do everywhere. They had worked out a calendar more accurate than the Julian one of Europe, observed the cycles of Venus, and produced tables capable of predicting eclipses. This accurate astronomy was combined with what we can call numerology for ritual purposes—something that would have seemed quite normal to Pythagoras or to the alchemists and astrologers of fifteenth-century Europe. Periods of time, and the numbers themselves, had symbolic or divine qualities, all of this in the setting of a

complex polytheistic religion that took a cyclical view of time and also of human history. My argument is, quite simply, that in terms of training, knowledge and intellectual sophistication the Maya priests compared well with the natural philosophers of medieval Europe.

My next case study is for technologists and it comes from coastal Ecuador where, nearly two thousand years ago, native craftsmen had empirically discovered how to make use of platinum as a jewellery metal. Although platinum cannot be melted by pre-industrial crucible-and-blowpipe technology, the problem was solved by a sintering process, i.e. by binding the grains of platinum together in a matrix of gold (rather like mixing sand into a small quantity of melted chocolate). This is the basic principle behind modern powder metallurgy, and the resultant mixture can be forged like any other metal (Plate 1). In passing, I might mention that when the Spanish conquered the region and began to exploit its gold-bearing gravels they threw away the platinum as a useless and intractable contaminant. As late as 1740 the only suggested uses for platinum in Europe were as small shot or in little bags for clock weights (McDonald and Hunt, 1982: 24)

The above information has long been available in the trade literature (e.g. Scott and Bray, 1980) written for specialists. For an alternative version of the story I now give you a passage from a book aimed at the general public, Erich von Daniken's *Chariots of the Gods*:

> How could anyone smelt and model platinum, since platinum only begins to melt at 1800°C?... Since we are not prepared to admit that there was a higher culture or an equally perfect technology before our own, all that is left is the hypothesis of a visit from space! (von Daniken, 1971: 44)

He should have visited the Institute of Archaeology, where we do the impossible all the time. My colleague, David Scott, once replicated an Ecuadorian sintered gold-platinum ornament with a cladding of pure platinum on one face. The process took him about one hour.

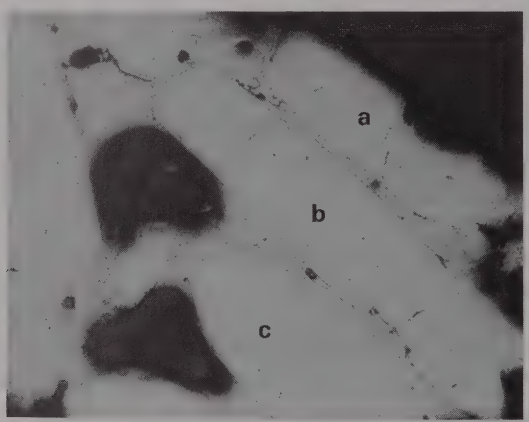

PLATE 1. Photomicrograph of an etched section through a sintered gold-platinum ornament from Esmeraldas, Ecuador. Flattened laths of native platinum (a, b, c) are bonded together in a matrix of gold (courtesy, David Scott).

Behind von Daniken's argument (and in the minds of those people who want to derive New World civilizations from Wales, Phoenicia, Egypt, China or wherever else) is the assumption that native Americans were incapable of inventing anything worthwhile for themselves. Von Daniken's books, nonsense though they are, sell more copies than any archaeological texbook, and there is clearly a long way to go in educating the non-specialist public.

The snippets of information I have just presented were designed to illustrate the point I made earlier, that the achievements of the prehispanic Americans are interesting for their own sakes. But does all this really matter in the real world? Who cares if the Maya invented the concept of zero, or that the Incas had one of the largest and best-run empires of the ancient world? Well, philosophers of science should care; so, too, should cultural historians, political theorists, and anyone else concerned with the history of ideas and with hypotheses about the development of social and political institutions.

Ancient America offers something unique, a historical trajectory that goes all the way from the Stone Age to a state of high civilization *without significant influence from the Old World*. Because prehispanic America is autonomous and independent, it allows a control experiment of hemispheric proportions.

Many disciplines take an interest in broad historical questions, for example: How, and why, did hunters and gatherers become farmers? How, and why, did the first pristine states emerge? Do all societies go through the same stages of development? Are there universal laws (or, at least, regularities) that govern the development of human cultures and societies everywhere? These are not trivial questions; they are mainstream problems that have interested Western scholars for more than two thousand years, from Greek philosophers to Marx and Engels. I am not going to try to answer these questions (and I am not even sure that there *are* answers) but I must point out that any general theories developed on the basis of Old World data can now be tested against the completely independent evidence from the New World. Does the model work for America? If not, the hypothesis will have to be scrapped; if the answer is 'yes', then we may be one small step nearer to identifying universal traits of human behaviour.

I would now like to broaden the terms of reference and to argue that pre-Columbian archaeology is just one facet of a much larger field of study—indigenous America as a whole—that is very much relevant to the modern world. The reasons for this are simultaneously historical, cultural and political. As George Orwell wrote in *Nineteen eighty-four*, that most political of novels, 'who controls the past controls the future: who controls the present controls the past' (Orwell, 1949: 199).

The politicization of the past, and especially of the non-European past, is nowadays a sensitive subject in archaeology (Gathercole and Lowenthal, 1990), and in the New World the act of conquest did not bring the culture and aspirations of native America to an end. The pyramids have been taken over by tourists, and the Indian kings have long gone, but the original Americans have not disappeared. Their 'collective memory' is a long one, and their prehispanic past is still a powerful symbol. For very different reasons the prehispanic

cultures are also significant to the politicians of the modern nation states that emerged after independence from Spain. These elements from indigenous culture, with their references back to a past that owes nothing to Europe, are one of the ways by which Latin America defines itself *vis-à-vis* the rest of the world.

There is a certain irony in the way that *ladino* nationalism has appropriated the symbols of the cultures it destroyed. Two recent events at Bolivia's most famous archaeological site (Tiwanaku, occupied during the first millennium AD) illustrate the way in which the battle to co-opt the past is still continuing.

> In 1990 the newly elected president...declared Tiwanaku a 'diplomatic capital' of Bolivia, and hosted a meeting of the presidents of the Andean republics within the temple precinct of the Kalasasaya, the ancient seat of Tiwanaku's kings. In 1992, a group of Aymara symbolically reclaimed their heritage by planting the *wipala*, a multi-colored flag emblematic of a unified Aymara nation, squarely and visibly on the summit of the Akapana pyramid. Their leaders declared unequivocally that the site belonged to the Aymara people, and not to the Bolivian nation. (Kolata, 1993: 302)

The past also leaves its visual mark on the present, and I can illustrate this by a quick tour around the city of Mérida, capital of the Mexican State of Yucatán, built by the conquistadors on (and, in part, out of) the ruins of the sixteenth-century Maya town of T'ho. Yucatán's most famous archaeological monuments are the imposing structures at Chichén Itzá, in particular its square, stepped pyramid (the Castillo) and a series of columns in the form of Quetzalcoatl/Kukulcan, the feathered serpent deity. Images of the archaeological Maya, often with specific references to Chichén Itzá, are ubiquitous in Mérida. They occur on nearly all the principal official monuments: the Palace of the Governor of the State of Yucatán (Pl. 2), the Palace of the City of Mérida (Pl. 3), the headquarters of the ruling national political party (Pl. 4), the Monument to Mexico (Pls 5 and 6), and on the buildings in the Park of the Americas (Pl. 7).

Overall, the message is a generous one. The public images, directed at the citizens of Mérida rather than at international tourists, emphasize the achievements of the ancient Maya and the contribution of their descendants to the making of Yucatán. There are also some frank references to the trauma of conquest and to the injustices done to the native population under the Colonial and Republican regimes. Mexico's ruling party, it must be remembered, has its philosophical roots in the revolution of 1910 and, in Yucatán at least, the official message is one of respect, conciliation, and a bicultural Yucatec identity.

Not everything is so politically charged. Pyramids and feathered serpents appear on walls all over the city (Pls 8 a–c) advertising mundane everyday goods: cement, ice, shirts, liquor, English lessons, and even computers. It gives a whole new meaning to the phrase 'pyramid selling'.

Some two million people still speak Maya, and in South America the Quechua language (spread from southern Colombia to Chile and Argentina during the expansion of the Inca empire) is spoken by at least eight million Indians. In two countries, Bolivia and Guatemala, the majority of the population (54%) is ethnically and culturally 'Indian', and for Ecuador

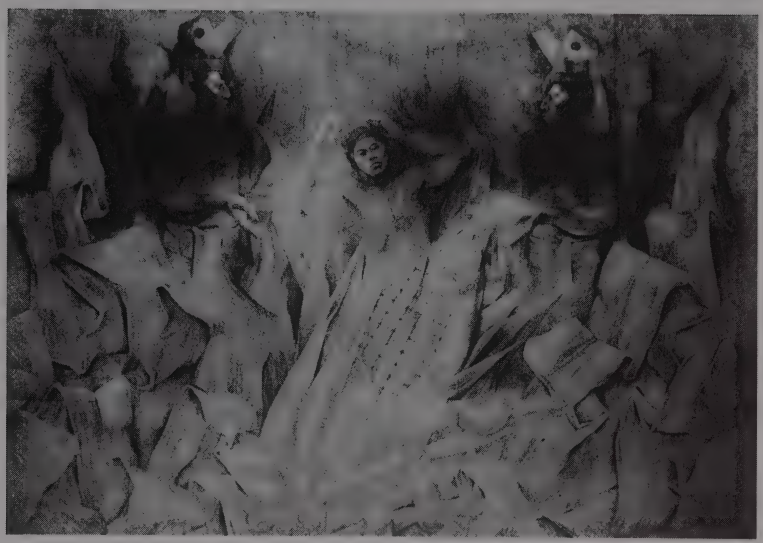

PLATE 2. Mural painting (by Fernando Castro Pacheco, 1978) in the Palace of the Governor of the State of Yucatán. It illustrates the Maya belief that the gods made humankind from maize.

PLATE 3. Mural painting in the arcade of the Palace of the City of Mérida. The architectural elements are based on structures at Chichén Itzá, and the composition links the ancient Maya with the revolutionary present (note the rifles).

PLATE 4. Supports copied from prehispanic originals at Chichén Itzá. These figures are part of a monument outside the Casa del Pueblo, Mérida, which is also the headquarters of the Partido Revolucionario Institucional. The main building has feathered serpent columns and pseudo-Maya reliefs.

PLATE 5. The Monument to Mexico, in Mérida (sculptor Romulo Rozo, completed 1956). The main personage is a pre-European Maya, and the container for the flame (between the legs of the figure) is in the form of a Maya house.

PLATE 6. The Conquistadors encounter the Maya on the Monument to Mexico.

PLATE 7. Feathered serpents in the Americas Park, Mérida.

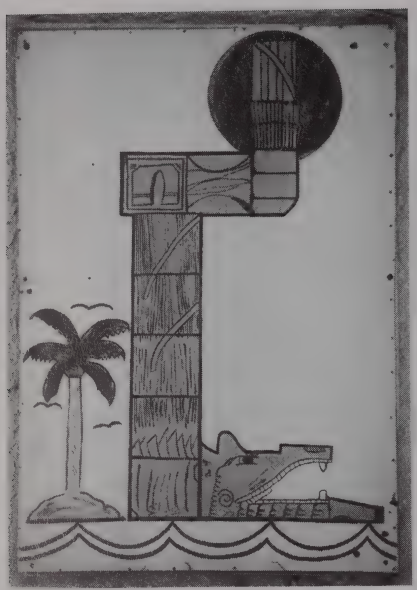

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PLATE 8. The Maya in advertising, Mérida 1992: a. academy for English lessons, b. ice, c. liquor.

and Peru the figure is over 40%. Most of these people live on the margins of the national culture and largely outside the state economy. In countries like Guatemala and Peru, where political violence is endemic, the central problem (how to incorporate the Indian population into a *ladino*-dominated state) brings the prehispanic past into brutal contact with the world of contemporary politics. Questions about how, or whether, to create a multicultural state are being settled with guns rather than by debate. And in these struggles the prehispanic past is still remembered.

Tonight I do not want to be drawn into political controversy; there are no easy solutions, and neither the Right nor the Left has much to offer the descendants of the Maya (Wright, 1989). Instead I will give just one example of the way in which the memory of the prehispanic past enters into today's rhetoric of discontent. In 1526 the conquistador Pedro de Alvarado captured and burned Iximché, capital of the Cakchiquel Maya kingdom, in the mountains of Guatemala. In February 1980 a predominantly Indian organization called the Peasant Unity Committee convened a meeting at the ruins and issued a document. One paragraph of the Declaration of Iximché (cited in Wright, 1989: 169) reads as follows:

> We the indigenous peoples of Guatemala declare and denounce before the world: more than four centuries of discrimination, denial, repression and exploitation committed by the foreign invaders and continued by their most savage and criminal descendants to the present day... The suffering of our people has come down through the centuries, since 1524, when there arrived in these lands the assassin and criminal Pedro de Alvarado.

The unfinished business of the Conquest is with us still.

I would remind you, too, that Guatemalans have won two Nobel Prizes. One, the Peace Prize for 1992, went to Rigoberta Menchú (a Quiché Maya) for her efforts to reduce the level of injustice and violence. The other, for Literature in 1967, was awarded to the novelist Miguel Angel Asturias, whose work combines surrealism with themes and metaphors from the traditional Maya world. His imagery is all but incomprehensible to a reader who has no knowledge of the indigenous past. Peace Prizes and manifestos such as the Iximché document bring the pre-Columbian world into direct contact with the present, and in broader terms the role of the Indian past in the world of today is a question that interests writers and painters, anthropologists, historians and politicians, as well as archaeologists.

My next example of a contemporary debate, and one that is getting a lot of publicity, concerns the best way to make use of tropical rainforests. It also allows me to tell a story from my own experience.

In 1977 I took part in a multidisciplinary investigation in one corner of the Amazon rainforest. One of our main aims was to look at the history of land use in the area from prehistoric times to the present. The archaeologists worked in collaboration with a group of ecologists who measured everything in sight: soil chemistry, the growth rate of plants in traditional Indian gardens and in government-sponsored manioc plots, nutrient needs of different crops, maturation rates and milk yields of cattle, and even the number of cow pats produced per animal per week. Each pat had its numbered peg and was visited daily until it disappeared. The local people thought we had all gone mad, but the ecologists came out with

unambiguous conclusions. Traditional Indian slash-and-burn farming, which looks so haphazard and untidy to conventional agronomists, proved to be a subtle and effective adaptation to rainforest conditions. So it should be, of course, for it is based on millennia of trial and error.

Meanwhile, the archaeological team was excavating fields and settlements going back to *c.* 1000 BC. Some of our sites were close to the river; others were on a plateau overlooking whitewater rapids. In the dark organic soils of ancient fields and house sites we found pot sherds, the stone axes used to clear the forest, and the carbonized remains of edible palm fruits. Laboratory analysis subsequently proved that these dark soils were anthropic, that fertility was enhanced by the incorporation of organic detritus from human activities, and that the presence of *terra preta* (black earth) soils was a good indicator of ancient settlement (Eden et al, 1984). More recent work by Colombian colleagues has pushed the story of Indian cultivation back to about 3000 BC, has identified pollen grains of maize, manioc and sweet potato, has demonstrated that the Indians were 'managing' the forest palm trees, and has shown that, by the eighth century AD, people were carrying up to the plateau huge quantities of riverine silt as fertilizer. It all adds up to some five thousand years of effective, stable, and ecologically non-damaging use of the forest (Herrera et al, 1992).

By 1977 we already had figures to prove that government schemes based on monocropping and cattle ranching were commercially non-viable in the short run, and were a long-term recipe for disaster. Feeling rather pleased with ourselves, and imagining that we had a contribution to make, we took our results to the appropriate minister (a graduate of the LSE) and explained that massive colonization of the forest by government-sponsored immigrants from the Andes simply wouldn't work. 'I know', was his reply. 'You're right, but I'm a politician and I have to offer them land.'

Not all experiences have been so discouraging, and archeology has had its successes. The setting for my final case history is the lakeshore plain around Lake Titicaca on the Peru-Bolivia frontier at about 3800 m above sea level. Today this land is of poor fertility and regarded as marginal for agriculture; it is badly drained, salt-impregnated, suffers from periodic floods, droughts and frosts, and serves mainly for low-grade pasture with some risky subsistence cultivation. This was not always the case. The air view in Pl. 9 shows a group of *camellones* (prehispanic ridged and drained fields), part of a system covering more than 800 km^2 of the Basin floor, which was abandoned well before the Inca conquest and remained completely unknown to Colonial and modern farmers.

Between 1981 and 1986, Clark Erickson (at that time a Ph.D. candidate at the University of Illinois) began to investigate the fields on the Peruvian side of the frontier in collaboration with a Peruvian agronomist (Erickson, 1985, 1986a and b, 1987; Garaycochea, 1986). Shortly afterwards a Chicago team carried out similar studies in the Bolivian portion of the Basin, as part of a project to examine the agricultural economy of the ancient Tiwanaku state (Kolata, 1991, 1993: chap. 6). Excavation showed that the earliest fields go back to the ninth century BC (a minimum age) and that the system attained its greatest extent and complexity during the Tiwanaku period, *c.* AD 400–1000. By this time the drained fields were just one element in a man-made landscape incorporating hillslope terracing, irrigation, canals, dams and aqueducts.

Having mapped and dated the relict fields, the archaeologists re-built some of them, planted the traditional high altitude crops, then watched and measured. The *camellones* consist of elevated planting ridges, usually *c.* 5–10 m wide and up to 200 m long, separated by canals or ditches that reach down to the water table (Fig. 1). The mounding produces a loose, well-aerated soil with a capacity to capture both water and air in micropores. This allows the growing plants to retain water and to absorb water-soluble nutrients. The ditches control both runoff and groundwater level. The water in the ditches also acts as a kind of storage heater, absorbing the sun's heat during the day and releasing it during the very cold nights. Daytime water temperature of the canals is several degrees above ambient air temperature, and measurements taken at the planting surfaces showed that the raised fields were some 2° Celsius warmer than the control plots, and that frost lay for a shorter time. On the *camellones* the growing season was extended; the plants on the ridges were raised high enough to avoid waterlogging, and the movement of water in the ditches flushed out harmful salts. Organic muck, water vegetation and fish manure from the canals provided a natural, and renewable, fertilizer, which eliminated the need for a fallow period.

The results on the experimental *camellones* were spectacular. Over a four-year period the potato yields (with no chemical fertilizer) were between 3 and 10 times the average for the Basin, and double those of control plots where fertilizers and pesticides were employed. In 1982, and again in 1988, when severe frosts destroyed most of the crops on conventional dry fields, damage on the reconstructed *camellones* was minimal.

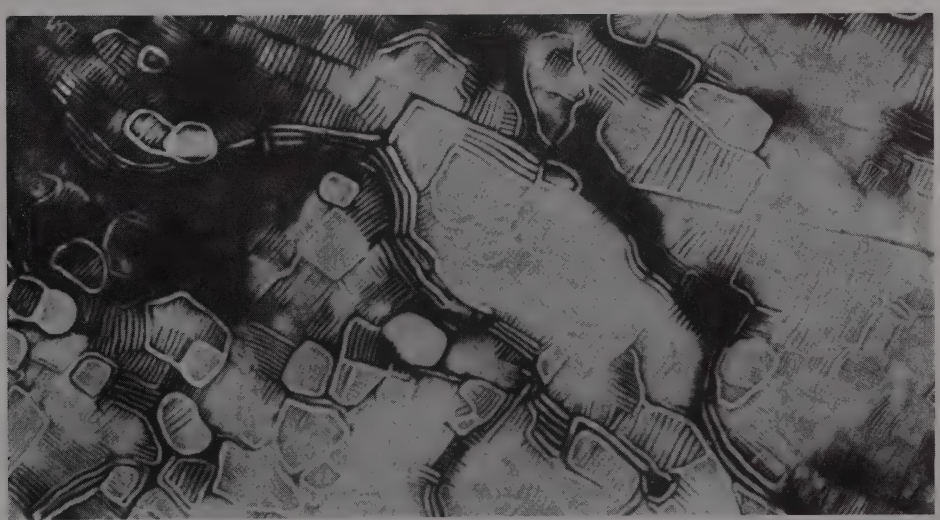

PLATE 9. Aerial view of prehistoric *camellones* on the edge of Lake Titicaca (after Denevan, 1985). The dark areas are water, and the light areas are the planting ridges (courtesy, Servicio Aerofotográfico Nacional, Peru).

Besides improving productivity, drained field cultivation is well adapted to the cultural and economic conditions of the Andean Altiplano. The region has a large workforce, but little capital is available for investment. There is also a tradition of communal labour that goes back to pre-European times. In this setting, the record of most Development Aid projects has been lamentable. Their Western-style remedies are capital-intensive (requiring chemical fertilizers, imported seeds, and machinery that nobody can afford), they do little to increase employment opportunities, and they concentrate on cash crops that benefit the farmers very little.

In contrast, the construction of drained fields demands little capital investment (no fertilizers or pumps) and relies on the resources the Altiplano already has to offer: abundant human labour, appropriate technology, and ready-made traditions of labour organization. Cultural and economic disruption is minimal, and results follow quickly. Once Erickson's project had shown the way, and had convinced the local farmers, the Peruvian government gave financial help and the scheme took off, with texts and picture books in both Spanish and Quechua and also a video training programme.

In the Pampa Koani region, in the hinterland of Tiwanaku, Kolata suggests that the ancient landscape could have sustained between 210,000 and 410,000 people with double cropping, and a minimum of 105,000 people with a single annual crop—this in an area that supports only about 7,000 people today, at a level slightly above bare subsistence (Kolata, 1991: 111). In these days when everything has to be directed towards the creation of wealth, the Titicaca Basin work is a model of applied archaeology, though I must emphasize that it began as a purely *academic* investigation.

This, I suspect, is only the start of things. In a completely different environment, at sea level in the Caribbean lowlands of Colombia, there are 5,000 km^2 of fields and canals,

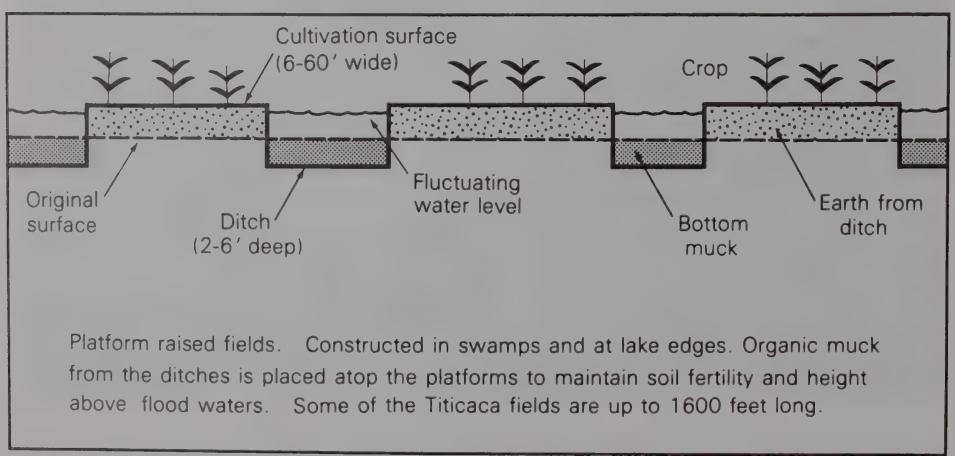
Cultivation surface
(6-60′ wide)
Crop
Original surface
Ditch
(2-6′ deep)
Fluctuating water level
Bottom muck
Earth from ditch
Platform raised fields. Constructed in swamps and at lake edges. Organic muck from the ditches is placed atop the platforms to maintain soil fertility and height above flood waters. Some of the Titicaca fields are up to 1600 feet long.

FIG. 1. Diagrammatic section through raised and drained fields (after Denevan, 1985).

abandoned some time around AD 1300 (Plazas et al., 1993). This is probably America's largest land reclamation scheme, and was unknown until the 1960s. There are expanses of drained fields in Mexico, Guatemala, Belize, Venezuela, Surinam, Ecuador, and the Bolivian lowlands, as well as Peru, and in several of these countries national governments are beginning to show interest (Farrington, 1985; de la Torre and Burga, 1986; Denevan et al., 1987). And in Peru alone there are two and a half million acres of terracing, most of it prehispanic. Over half of this lies abandoned today (Denevan, 1985), though experiments have shown that restoration is often feasible. Perhaps, after all, there is an alternative to reckless felling of the rainforest. Native American agriculture, and also indigenous pharmacologies, are becoming a serious business in the modern world.

Up to this point in my discussion I have been trying to show that archaeology in Latin America does not exist in isolation, but contributes to, and benefits from, dialogues with other disciplines. In the final section of my lecture I would like to look forward towards the future of American archaeology within the British university system.

If you have been following my arguments you will already have guessed what my priorities are. I want to see the New World given the place it deserves in archaeology syllabuses, and, secondly, I would encourage closer relationships with other subjects in order to bring archaeology back into the mainstream of Latin American studies. These two objectives are to some extent incompatible. The first of them requires the strengthening of the subject *within* archaeology departments; the second requires archaeologists to spend much of their time *outside* these departments.

It is easy enough to identify problems, but much more difficult to come up with solutions. My first question is, simply, 'How many teachers of American archaeology does Britain need?' The answer, clearly, is 'Not very many', but (after almost thirty years of benign neglect) I do assure you that we need more than one. There are several reasons for saying this.

One is that I believe it is intellectually bad for students to be exposed to only one voice, one person's database, and one view of the subject. I have to say that, as a teacher, this unwished-for monopoly has weighed heavily on me. None of us is perfect, and two sets of prejudices are better than one.

My second argument is that the subject (what there is to read and what there is to know) is growing and developing at a frightening rate. My old mentor Geoffrey Bushnell personally knew almost every archaeologist in his field, and once gave a party for the entire International Congress of Americanists in his back garden in Cambridge. Attendances at recent meetings have averaged between 1,000 and 2,000 people. When I began, in the 1960s, it was still possible—more or less—for one person to keep up with the reading and to teach most of what was required. This is not so today. As the discipline of American archaeology grows, while teaching and research resources are held constant, standards are eroding, imperceptibly, a little bit each year, as a smaller proportion of the total corpus of knowledge is passed on to each successive generation of students. This is no way to build up a strong school of Pre-Columbian Archaeology in Britain, a school that can compete with the large, well-

funded departments in the United States and elsewhere. We are in danger of losing not only our intellectual credibility, but also our place in the international market.

It makes no sense to scatter Americanists in ones and twos all over Britain. The need is for one, or at most two or three, centres of genuine excellence. Being realistic about the present state of university funding, any department that employed two Americanists would move straight to the top of the list,[2] and any department that took on three of us would corner the market. This is not too much to hope for.

My second problem has more to do with organization than with funding. I have been arguing that if Latin American archaeology is to be seen to be useful in today's world it must exploit its natural links with other disciplines. I think this view is shared by the Institute of Archaeology, and I hope also by University College. One difficulty is that many of the things that must be done are frustrated by administrative boundaries created by historical accident as universities have developed and expanded. In London the problem is exacerbated by the university's federal structure. If I could write my own ideal syllabus I would want all would-be Americanists to have access to courses in anthropology, ecology and geography, colonial history and literature, ethnohistory and indigenous languages. I have chosen this list carefully. Every item on it is, or recently has been, on offer somewhere in London, though not always at University College or at undergraduate level.

What happens at the moment is that universities are producing two kinds of people—archaeologists who know a bit about Latin America, and Americanists who have taken a course or two in archaeology. This half-and-half training is far from ideal, and we must find some way of bringing the parts together into a coherent whole. In many places in Britain, and especially in London, where my colleagues at the (multidisciplinary) Institute of Latin American Studies have been a constant source of inspiration, the necessary resources already exist. I am not, therefore, fantasizing or asking for the unaffordable, but rather for increased flexibility between disciplines and between institutions, and also for bureaucratic tolerance towards the awkward, one-off, student who does not fit the conventional pattern. We may not need *many* Americanists, but we do need *good* ones.

And with that I rest my case.

Abstract

This inaugural lecture argues for the importance of New World Archaeology within the field of archaeology as a whole. Ancient America offers something unique (a long trajectory from Pleistocene foraging to high civilization) and this allows hypotheses developed in the Old World to be tested against a completely independent database. These questions should be of interest to all disciplines concerned with social theories and the history of ideas. The study of native America is also relevant to the modern world; prehispanic techniques of land-management are being revived by today's governments, and the politics and attitudes of contemporary Latin America cannot be understood without reference to the pre-European past. The neglect of American archaeology in British universities must be rectified.

Notes

1. This paper was originally delivered as a lecture. I have added references, included one or two afterthoughts, and made the adjustments necessary when a verbal presentation is turned into a written one. I am grateful to Stuart Laidlaw for converting several of the lecture slides into illustrations for this paper.
2. In September 1994 the Institute of Archaeology appointed a second lecturer (Dr José Oliver) in Latin American Archaeology. My text, therefore, represents the situation as it was in 1993.

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Childe in History†

by PETER GATHERCOLE*

> He was very ugly, and he was an Australian... He was an Outsider.
> (Piggott, 1958a: 310)

In 1924 Lenin died, opening the door for Stalin's path to supreme power in the USSR; Britain had its first (shortlived) Labour Government, and Rajani Palme Putt, sometime Balliol classical scholar, Childe's Oxford friend during World War I, and foundation member of the Communist Party of Great Britain, married Salme Pekkala, a Comintern 'representative' living illegally in England. The couple moved to Brussels, where both worked for the Comintern (Page Arnot in S. Dutt, 1966: 8–9; Callaghan, 1993: 41–2). These were uncertain times. And what of Childe? He had no settled job, living at the Bloomsbury House Club in Cartwright Gardens, near to this Institute, writing, and, when opportunity and money allowed, visiting sites and museums on the Continent (Green, 1981a: 40–8). He was finalizing the book that was to bring him wide academic approval, *The Dawn of European Civilization*, when it was published in the prestigious series, *The History of Civilization*, by Kegan Paul, the following year (Childe, 1925a). 'It contains errors, both in fact and interpretation', wrote W.J. Perry in *The Nation and the Athenaeum*, 'but these do not weigh in the scale against its great value as a convenient guide to the subject as a whole' (Perry, 1925).

It was not only in archaeological circles that Childe was becoming known for his writings. In 1921, with Comintern money, Dutt had founded *Labour Monthly*, a left-wing journal (Callaghan, 1993: 43–4), where Childe published two articles on the Australian political scene, one under his own name (Childe, 1924), but the other 'by an ex-ruler' (1922), because he was still then in the employ of the New South Wales Government in London. His post as Research Officer had been abolished, following a change of government, shortly after he arrived in London from Sydney, although he was allowed to stay employed for a few months. In the next year appeared *How Labour Governs: A Study of Workers' Representation in Australia* (Childe, 1923). It was published by the Labour Publishing Company, part of the Labour Research Department, a left-wing organization for whom Childe had done some work while at Oxford, and which he now re-joined (Irving, n.d.: nn. 1, 53). The LPC was a brainchild of G.D.H. Cole, Oxford academic and prominent Labour Party intellectual, who had been President of the University Socialist Society when Childe was Secretary. Cole, his wife Margaret, her brother Raymond Postgate and others in that Oxford/LRD wartime grouping welcomed Childe back into left-wing circles after his return to England,

† The sixth Gordon Childe Memorial Lecture, given on 23 February 1994.

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where, according to J.G. Crowther, who first met Childe at an LRD summer school, he was known as a political historian (Crowther, 1970: 20).

Childe, the political radical, one-time private secretary of John Storey, the Labour Prime Minister of New South Wales, was, however, still on the fringe of the English archaeological world. Over the next thirty years, he was to become a most distinguished European prehistorian, submerging the memory of the radical Australian element in his life—at least to his archaeological colleagues and students. By the early 1950s he was, in the eyes of many, an august figure, somewhat remote and eccentric, whose features could be lampooned readily, albeit affectionately (Pl. 1). He was often thought of as eminently aged. In February 1950 Michael Oakeshott, editor of *The Cambridge Journal*, wrote to Glyn Daniel concerning an unsolicited article Childe had submitted on Prehistory and Marxism (later published by Daniel in *Antiquity* (Childe, 1979)). Oakeshott said he did not have room for it, and asked Daniel, having read it, to return it to the author. 'Send the old boy an encouraging note when you return his stuff' (Oakeshott, 1950). Childe was then 57.

The details of Childe's archaeological career are well known, thanks largely to the books by Trigger (1980), McNairn (1980) and Green (1981a), which collectively have been the basis for serious historiographic study of the man and his professional career. In acknowledging my debt to their researches, my approach, as the title of this lecture indicates, is somewhat different. I wish to see Childe as a historical being, rather than as a prehistorian in a historical context (which of course in no respect reflects ill on the researches I have just mentioned). In recent years, however, in addition to publications by archaeologists which have either added new information (e.g. Makkay, 1991a, 1991b; cf. Childe, 1991) or increased our understanding of Childe's longer term significance (e.g. Sherratt, 1989; Lech, 1992), research in Australia and the United States has provided a much fuller picture of Childe's place in Australian political thought (e.g. Irving, 1988; Melleuish, 1988) and of his relationship to American anthropology (Peace, 1988). Publication of the proceedings of no less than three conferences marking the centenary of Childe's birth in 1892, held in Brisbane in 1990 (Gathercole et al., forthcoming), at this Institute in 1992 (Harris, 1994), and in Warsaw in the same year (Lech and Stępniowski, forthcoming), can only enhance our knowledge in all these areas. Moreover, the opening up of the archives

PLATE 1. Childe at Cambridge, 1952; drawn by Jacques Nenquin, then a research student at Trinity College (courtesy, Dr J. Nenquin).

of the Communist Party of Great Britain has stimulated research on left-wing history in this country which has bearing on Childe's relations with, for example, Rajani Palme Dutt, illuminating aspects of his archaeological, as well as political, attitudes (cf. Callaghan, 1993; Morgan, 1993). I have been able to draw directly on this large amount of new material only to a limited extent in the preparation of this lecture, but naturally it has influenced the way I have approached my theme.

After Childe's sudden death in October 1957, there was wide expression of the view that he was shy and aloof, living essentially for scholarship. 'It was impossible to get really close to Gordon Childe,' said Edward Pyddoke, then Secretary of this Institute, in a broadcast tribute (Pyddoke, 1958). Piggott's magisterial yet affectionate obituary in the *Proceedings of the British Academy* said it all: 'The fact that he was the dedicated scholar, as soul-hydroptic for learning in the archaeological field as was Browning's Grammarian in his, in itself poses a problem. So far as anyone knew him, he appeared simply as an embodiment of his work, engrossed in pursuing his constant intellectual course' (Piggott, 1958a: 310). Whatever the validity of this assessment (and it is fair to add that tributes from former students, e.g. Stewart Cruden (Cruden, 1957), presented a broader picture), within the Left, in both Britain and Australia, Childe has been remembered as a more relaxed and informal, though still detached, individual (J. Jenkins, personal communication 1971). Jack Lindsay, a fellow Australian who later knew Childe in Communist circles in London, remembered him in Brisbane in 1919 'as a bubble-pricker, a mildly caustic iconoclast...the most detached person I knew' (Lindsay, 1958: 135). The founders of the journal *Past And Present* could write of him as 'in his time the most distinguished man in Marxist history anywhere... Some of the earliest meetings of the Board were held at his flat. We admired this lonely, ugly, friendly man who watched television in a gothic pub on Tottenham Court Road' (Hill et al., 1983: 13). Clearly Childe was a more complex, more contrary, man than these inevitably partial reflections allow.

However, Piggott's picture of the single-minded scholar, following his own academic programme regardless of personal cost, was to a degree confirmed by Childe's self-portrait in 'Retrospect', the autobiographical fragment published in *Antiquity* shortly after his death (Childe, 1958a). Here was the clear message that the man had known where he was going over the previous thirty years—and in a direction (advancing 'interpretative concepts and methods of explanation') which colleagues had not anticipated (cf. Daniel, 1958; Piggott, 1958b). In addition, there was, overall, the nebulous atmosphere of incompleteness, of eccentric irresponsibility ('an innocent abroad' wrote Mallowan (1977: 234)), of failure almost. When Childe wrote, in a throw-away phrase, of his earlier career in Australian politics as 'a sentimental excursion' (1958a: 69), and in the statement written shortly before his suicide that he had 'lost faith in all my old ideals' (Childe in Daniel, 1980: 3), how therefore was the balance to be struck, if indeed it had to be, between greatness and failure? Subsequent assessments of the longer term significance of his work, especially those stressing either the inhibiting influence of his Marxism (Clark, 1976:3–4; Walker, 1983) or the extent to which his writing had been superseded (Sherratt, 1989: 153), served only to emphasize the seriousness of the question. To see Childe 'in History' may perhaps help to answer it.

A MAN IN A HURRY

'Retrospect' (not, incidentally, his title; Childe left the piece untitled) reads as the record of a stately and successful progression of achievement from one academic goal to the next. It was, one must remember, the statement of a 65-year-old man on the eve of death, looking backward. In practice, things were not like that. The issues were not so clear cut. Irving has argued convincingly that *How Labour Governs* and other writings of 1922 to 1924 were neither the signal of Childe's disillusionment with Australian Labour politics as such, nor simply an indictment of Labor Party careerists, but a perceptive appreciation of the Party's dual role as one of representation and as an agent of mediation between class and nation (1988: 76, and also below p. 36).

By the same token it can be maintained that Childe did not regard his move into professional archaeology as the disillusioned rejection of political activism (cf. Gathercole, 1989). Dutt's statement that Childe went to the Abercromby Chair at Edinburgh in September 1927 because he found the price of revolutionary politics 'too high' (R.P. Dutt, 1965) indicates not only that up to the age of 35 he still saw a political career as a possible option, but also that other forms of political activity were still open to him. (Indeed the next year he joined the Association of Scientific Workers, later becoming the Edinburgh Branch Chairman and, in 1946, a member of its National Executive Committee (Wooster, 1958)). Times were still uncertain. Political action could not be totally abjured. Childe's academic career was carried through in the context of wars, revolutions and counter-revolutions, slumps, and latterly the divisions of Cold War Europe (cf. Childe, 1958c: 304–5). His Europe of research was a central political cockpit, and for considerable portions of his life was closed to him.

Thus when, at last, Childe was able to embark on an archaeological career as an Edinburgh professor, it was not just his age that reminded him that he had no time to lose. He had to justify his appointment, which, one should remember, had not been his for the asking—both Burkitt (Daniel, 1986: 66–7) and Wheeler (Hawkes, 1982: 108) had declined to apply. Moreover, in a sense by 1927 Childe had run out of his initial supply of archaeological-cum-intellectual capital. *The Dawn* (1925a) and his next book, *The Aryans* (1926), surely came from the reservoir of knowledge that included his Oxford B.Litt. thesis and at least some of the results of his visits to sites and museums since 1922; this was expressed also, for example, in his article on the Beaker Folk (Childe, 1925b). Moreover, *The Danube in Prehistory*, that enormous tome which dominates Childe's publications of the 1920s, though not published until 1929, was completed two years previously, just before the move to Edinburgh (Childe, 1929a: xii). Indeed, much of it must have been formulated at the same time as the earlier books (cf. Makkay, 1991a: 108, 114 n. 6). So one could suggest that Childe assumed his Chair with a clean slate. He not only had to be in a hurry; he had the opportunity to be so.

Childe's next book *was* written quickly; *The Most Ancient East* (1928a) was based on a course of lectures, given presumably in London in 1927 and later in Edinburgh (Childe, 1934a: xvi), which consisted of a survey of recent discoveries in the designated area. In rather similar genre was *The Bronze Age* (1930a), commissioned by Cambridge University Press as a companion volume to Burkitt's *Our Early Ancestors* (1926)—written to appeal 'to

the same class of students' (Childe, 1930a: xi). Although it is certainly true that Childe published five major books in six years, they differed from each other in significance; those published in 1928 and 1930 were responses to immediate demands.

Such was even more true of *Skara Brae: A Pictish Village in Orkney* (1931c). Soon after his arrival in Edinburgh, Childe was, in his careful phrases, 'privileged to be present during the operations undertaken by [H.M. Office of Works] for the conservation of the remarkable prehistoric village of Skara Brae and to supervise the incidental archaeological results on behalf of the Society of Antiquaries of Scotland' (ibid. v). Conservation work had begun before Childe arrived in Scotland; he had received no training in excavation (his field experience at that time might be said to have comprised some work in Hungary), and he was completely new to the details of Scottish prehistory. But, as so often subsequently during his Edinburgh years, he responded by transcending the demands of the immediate event. His sequence of publication concerning this site could not have been more rapid: yearly interim reports in the *Proceedings of the Society of Antiquaries of Scotland* (Childe, 1929b; Childe, 1930b), culminating in two reports and a book in 1931 (Childe, 1931a, 1931b, 1931c), as well as two other earlier incidental articles (Childe, 1928b, 1929c, cf. Pl. 2). As Trigger has said, 'since this was Childe's first significant archaeological excavation, and he had few good models to follow, it augured well for his future as a field archaeologist' (1980: 80).

PLATE 2. Skara Brae, Orkney, receives recognition from a British Labour Government, August 1929: (a) Childe at the site with the Rt Hon. George Lansbury P.C., M.P., First Commissioner (i.e. Minister) of Works (courtesy, Royal Commission on the Ancient and Historical Monuments of Scotland); (b) Childe, Lansbury and Raymond Postgate, his son-in-law, outside Stromness Museum (courtesy, W.H. Hourston, courtesy, Orkney Library Collection).

Between 1928 and 1938 Childe undertook much fieldwork in Scotland (and some in Ireland), while during some of the War years he devoted a considerable amount of time to rescue survey (Graham, 1970: 278, 1978: 302, 308–11, 347–8, 1981: 223–4). The results of these activities were published promptly, as bibliographies of his writings indicate so convincingly (Smith, 1955; Green, 1981a: 176–90; Gathercole et al., 1990).

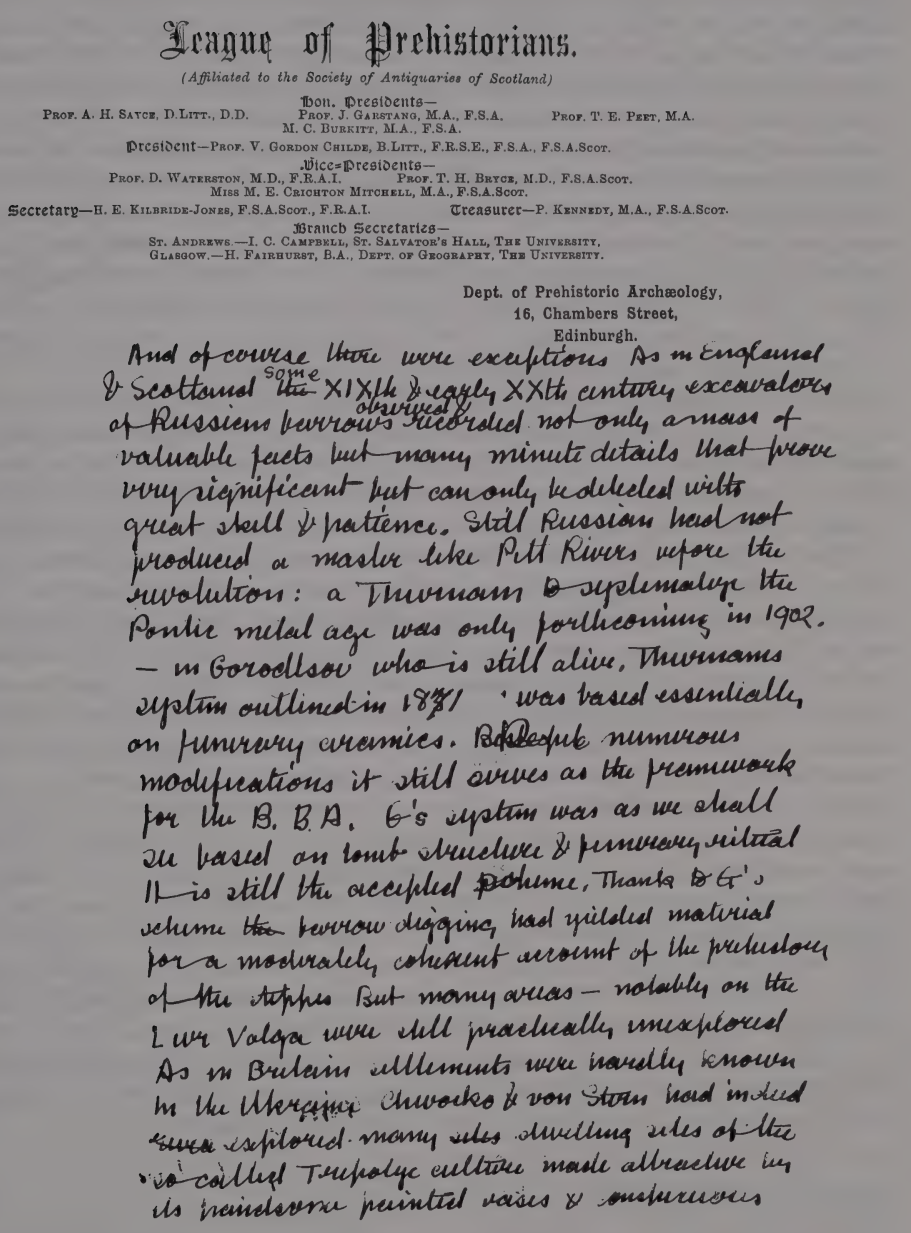
Trigger has remarked on the amount of innovation evident in Childe's Scottish research (1980: 77–90), which I need not recapitulate here. More relevant to my particular argument is the fact that Childe repeated the practice initiated with *The Most Ancient East* and its successor, *New Light on the Most Ancient East* (1934a), by drawing together the results of his and others' activities in *The Prehistory of Scotland* (1935a) and in a Historical Association monograph, *Prehistoric Scotland* (1940c). The general reader was never very far from his thoughts.

In much of this work Childe took advantage of situations as they presented themselves—in Skara Brae style. Money was always limited, helpers usually few (though presumably work for the government was better provided for), and schedules tight. 'Childe by the way, looked in here 2 days ago,' wrote Mansfield Forbes, Fellow of Clare College, Cambridge, to H.M. Chadwick on 25 May 1935 '—motoring N. to Edinburgh. He was in good form, (after weeks of trouble with bad throat). He is excavating in July in Galloway—a site which will subsequently be drenched under the Galloway Hydro-Electric scheme (La Tène-ish site, he thinks)' (Forbes, 1935). One solution attempted for a time was the League of Prehistorians, its name seemingly punned from the Young Communist League (Green, 1981a: 67), with an impressive array of honorary officers listed on its headed notepaper (Fig. 1). But such support was not sufficient to sustain the League, and it seems to have folded after 1935 (Trigger, 1980: 81).

Childe tied his personal way of life to this professional pattern. It seems that he never owned his own house or flat. During 19 years in Edinburgh he first lodged in the pleasant suburb of Liberton, and then lived for many years at the appropriately named Hotel de Vere in the West End, next to New Town. His appointment enabled him to travel a good deal, when not teaching or in the field, and to that degree he remained an Outsider, forever looking to the horizons of his subject.

Although he won friends and supporters for the cause of prehistory (R.B.K. Stevenson, personal communication 1990), as in Australia he encountered, especially in his early years, resentment and political prejudice, which hard work and an impressive publication list did not always dispel. Angus Graham, from 1935 Secretary of the Ancient Monuments Commission of Scotland, has a revealing anecdote concerning the attitude towards Childe held by Sir George Macdonald, its Chairman (who 'stood at the head of every conceivable tree, scholastic, antiquarian and official' (Graham, 1981: 213)). With war imminent, Sir George agreed that O.G.S. Crawford, Archaeology Officer to the Ordnance Survey, should be informed about an archaeological matter with a security aspect; not, however, Childe. ' "But if Crawford, why not Childe?" I asked; "they're both Communists." "Yes," he replied, "but Crawford's not an ass" ' (ibid. 223).

The threat of war brought a unique opportunity. It was the major stimulation for the total



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And of course there were exceptions As in England & Scotland the same XIXth & early XXth century excavators of Russian barrows recorded obviously not only a mass of valuable facts but many minute details that prove very significant but can only be elicited with great skill & patience. Still Russia had not produced a master like Pitt Rivers before the revolution: a Thomsen & systematize the Pontic metal age was only forthcoming in 1902. — in Gorodtsov who is still alive. Thomsens system outlined in 1837 was based essentially on funerary ceramics. Despite numerous modifications it still serves as the framework for the B. B. A. G's system was as we shall see based on tomb structure & funerary ritual It is still the accepted scheme. Thanks to G's scheme the barrow digging had yielded material for a moderately coherent account of the prehistory of the steppes But many areas — notably on the Lwr Volga were still practically unexplored As in Britain settlements were hardly known In the Ukraine Chvoiko & von Stern had indeed even explored many sites dwelling sites of the so called Tripolye culture made attractive by its painted ware painted vases & enormous

Fig. 1. The headed notepaper of the League of Prehistorians, early 1930s, with a sample of Childe's Edinburgh Lectures (courtesy, Institute of Archaeology, UCL).

re-writing of *The Dawn*, completed in September 1938 (Childe, 1939a: xvii–xviii). More significant was one of Childe's reactions to the outbreak of the war itself. Within a few months he was busy on a text surveying recent research in the whole British Isles (though his direct experience of excavation in England and Wales was limited). Again he worked fast, for who could predict what destruction the conflict might bring? The threat of German invasion in the summer of 1940 made him anxious for both the manuscript and his own life (R.B.K. Stevenson, personal communication 1990). So chapter by chapter a copy was sent to Hugh Hencken at Harvard (Peace, forthcoming). Happily the book, *Prehistoric Communities of the British Isles*, appeared in the following October with this hopeful sentence in its Preface: 'The war, having largely arrested discovery, offers a breathing space for digestion' (Childe, 1940b: v). The new *Dawn* was not so fortunate; most of the edition was incinerated during the London Blitz of the winter of 1940 to 1941, calling for a new edition as soon as fresh material became available after the war (Childe, 1948: xix).

As he had anticipated in a letter to Crawford in December 1944 (Childe, 1944f), Childe was pleased to leave Edinburgh in 1946, not least because in London he was able to be in more regular contact with friends, non-academic as much as academic, including those on the Left. To head the Institute of Archaeology as well as holding a Chair, despite his distaste for administration (and inadequacies in that respect), was very rewarding. The last decade of his life was rich in completed research and attendant personal satisfactions. He taught more students, including those at higher levels, and maintained his accustomed publication rate, its field much wider than hitherto. On the recommendation of Agatha Christie, the novelist and wife of Max Mallowan, he moved into Lawn Road Flats in Belsize Park, Hampstead, designed in 1934 by the innovative architect, Wells Coates, for 'the new type of man who likes not only to travel light but to live light' (Richards, 1958: 359), a description that could have been coined for Childe personally. The Flats became a gathering place for 'the Hampstead artygentsia' (MacCarthy, 1992), which may have appealed to him.

Some of his academic writing revealed a political edge. Although there are elements of subjective Marxism in, for example, *What Happened in History* (Childe, 1942d: 12–17), this trend became more evident in several post-war publications: *History* (1947) and those dealing with the sociology of knowledge (Childe, 1949a, 1949b, 1956a) being the most obvious. The habit of writing fast remained. He decided to retire a year early, aged 64, in 1956, and there were several books to be written before then. *Society and Knowledge, Piecing together the Past* (1956c), *A Short introduction to Archaeology* (1956d), and a revised edition of *Man Makes Himself* (1956e), all appeared in that year, followed by the sixth edition of *The Dawn* (1957) and its popular counterpart, *The Prehistory of European Society* (1958d). This astonishing achievement suggests that Childe worked to a planned writing programme, to be completed before his final return to Australia.

THE POPULAR WRITER

Childe was always interested in popular causes, and in helping to popularize them, however unpopular such activities might be to some people. What else, in one sense, was *How*

Labour Governs? The book reads austerely, yet it was more than a treatise for specialists. The archaeological works of synthesis discussed in the previous section were also directed at a non-specialist readership, though none were expected to achieve mass sales. Within the Left, with its constant demands for popular education, Childe's contributions to *The Plebs*, the journal of the National Council of Labour Colleges, amounting to two articles and 17 reviews between 1923 and 1944, were popular pieces in a literal sense. Here, for example, are some characteristically robust comments on Haddon's *History of Anthropology* re-issued in 1934: 'Any science springs from practical needs and in its theoretical development reflects changes in the economic system...[the] History of Anthropology...provides materials for a study of the subject on Marxist lines, but naturally lacks space to expound the bearing of the economic background upon the several phases of the science's evolution' (1934b).

Of course, Childe's most popular books were *Man Makes Himself* (1936), *What Happened in History* (1942d), and—a long way behind—*Progress and Archaeology* (1944c). The latter was re-printed only once from its original edition, but each of the first two have been re-printed many times, and are still available. Both have been widely translated. Mulvaney once wrote that Childe 'was probably the most prolific and the most translated Australian author' (1957: 93). This honour should perhaps go to Jack Lindsay, but surely Childe has been the most widely read archaeological author writing originally in English. It is impossible to obtain figures of their sales, but Childe's own figure of over 300,000 copies of *What Happened in History* sold up to 1957 (Childe, 1958a: 73) indicates the likely numbers involved. To discuss the content of these books is not my intention; this has been well done by Trigger (1980: 104–14) and by Green (1981b). My interest here is in their socio-political contexts. The basic theme of each is social evolution, encompassing the neolithic and urban revolutions in the Old World, with the account being taken up to the end of the Roman Empire in *What Happened in History*. The earlier book was published by Watts and Co., associated since its foundation in 1899 with the Rationalist Press Association, and it possesses this rationalist hallmark. Archaeology is a science knowable by rational means; it can provide the data for a non-metaphysical and coherent account of social progress—which can be published in an inexpensive volume. All was summed up in the book's title, taken, I think, from some remarks by a 'Man of Letters' in G. Lowes Dickinson's *A Modern Symposium* (1905: 152): 'Man is in the making; but henceforth he must make himself.' Appropriately, in 1941 the book became part of the RPA's 'Thinker's Library', when Childe became an Associate of the Association. It maintained a steady sale through the war years. (In June 1941 there were reports in *The Plebs* of a successful day school in Nottingham, where Childe lectured on 'social development' to an audience of 170, and 45 copies of the book were sold.)

The genesis of the book fits this history, referred to obliquely by Childe in 'Retrospect' (1958a: 72). Asked by his friend J.D. Bernal, shortly to become Professor of Physics at Birkbeck College, London, to contribute to a projected Marxist History of Science, Childe produced at least two drafts on the origins of civilization, which clearly lie behind the text and ideology of *Man Makes Himself* (Bernal, n.d.). The book was written fast, and some of it

has a rather slapdash air. For example, on page 2 there are unreferenced quotations from works by Karl Pearson and Hyman Levy, actually derived from the latter's *The Universe of Science* (1932: 179, 189). The remarks by Levy include reference to the number of words on one page of his unnamed book; surely inappropriate, if not incomprehensible, when removed from its original context.

Childe's mention of Levy is interesting in a more profound sense. He was one of a group of scientists, including Bernal, J.B.S. Haldane, J. Needham and L. Hogben, who in the 1930s were increasingly active in the field of the social and political responsibilities of scientists (Werskey, 1978). Several of them were also members of the Tots and Quots, the dining club founded by Solly Zuckermann in 1931, of which Childe was also a member (Zuckermann, 1978: 60–1, Appendix 1). *Man Makes Himself* can be associated in its philosophy with the attitudes of the members of Werskey's 'Visible College', stressing as it did the need for a scientific approach to the study of the past, and posing an objective science in opposition to the growing danger of fascist obscurantism (Childe, 1936: 2). One of the powerful influences on the writings and activities of these men was the contribution of the delegates from the USSR to the Second International Congress of the History of Science and Technology, held in London in 1931 (Werskey, 1978: 138–49). The geneticist, N.I. Vavilov, spoke at the meeting (Vavilov, 1931), and it may be significant that Childe mentioned the latter's work in his book (Childe, 1936: 77).

There are traces of scientism in *Man Makes Himself*. Childe wrote of the 'realist' as distinct from the materialist conception of history (ibid. 7, cf. 1935b: 9–10), a term borrowed from G.D.H. Cole's *What Marx Really Meant* (1934: 14–16; McNairn, 1980: 104–5). This tendency to equate Marxism 'via' realism with scientific objectivity was consistent with the views of Bernal, who saw the future in terms of the twin triumphs of science and socialism. The danger of this attitude in relation to Marxism was well stated by the classicist, Benjamin Farrington, who much admired Bernal: 'It seemed to them [the scientists]...that Marxism was the theory which gave science its opportunity... And it seemed as if science and Marxism had absolutely been married to one another—they were the same kinds of things' (quoted in Werksey, 1978: 250). It 'was the "scientism" of Bernal and his associates' (ibid. 251) which Farrington found objectionable. In 1938 Childe regarded *Man Makes Himself* as a Marxist work (see p. 38), although this was not necessarily the orthodox Marxist view in Britain. In 1949 George Thomson characterized it as a statement of bourgeois empiricism (Thomson, 1949: 266). The charge of 'scientism' would conform to this assessment.

What Happened in History was completed just as rapidly as *Man Makes Himself*. First considered by Penguin Books for its Pelican Series in January 1938 (P. Sarson, personal communication 1976), the book can be seen as a contribution to the war effort, in that Childe wrote it to convince himself, in a phrase that has impressed commentators since it appeared, 'that a Dark Age was not a bottomless cleft in which all traditions of culture were finally engulfed' (1958a: 73). Letters to Crawford in 1941 (Childe, 1941b, 1941c) chart progress in writing, including the need to reduce its length. Poorly produced because of paper restrictions, it was published in 1942, to join other Pelicans that acquired a strong popularity

among members of the military forces. Richard Hoggart has commented recently on the significance of these books: 'We had a kind of code that if there was a Penguin or Pelican sticking out of the back trouser pocket of a battledress, you had a word with him because it meant that he was one of the different ones [i.e. someone seeking]...a change in climate which finally produced the Labour Government of 1945' (Hoggart, 1993: 14). *What Happened in History* had a special place in the affections of members of the Communist Party. 'It *was* prehistory for us,' Margot Heinemann, a long-standing member, told me in 1990. I remember the book being on sale, essentially to a British Army clientèle, in Egypt in 1947–9. There are still copies around the Middle East; according to press reports Brian Keenan read one while a hostage in Beirut.

Childe wrote the book with the same absence of reference to sources as in *Man Makes Himself*, presumably because he considered them unnecessary in such a work. But his use of Heichelheim's compendious *Wirtschaftsgeschichte des Altertums vom Paläolithikum bis zur Völkerwanderung der Germanen, Slaven und Araber* (1938) without proper acknowledgement has been condemned as culpable by Green (1981a: 98) and also criticized by Trigger (1994: 117). Heichelheim is named once in the book (Childe, 1942d: 232) but without citation. To say that other authors receive similar treatment—e.g. Farrington (p. 201); Glotz (p. 208); and Rostovtzeff (pp. 204, 226–7) (but cf. p. 246, where one of the latter's articles is both quoted and referenced)—is no excuse. For the great majority of Childe's readers, however, the issue of plagiarism did not arise, the message of the book being encapsulated in its last paragraph: 'Progress is real if discontinuous. The upward curve resolves itself into a series of troughs and crests. But in those domains that archaeology as well as written history can survey, no trough ever declines to the low level of the preceding one, each crest out-tops its last precursor' (Childe, 1942d: 252).

MARXISM

> My dear Raji, A pity that we could not meet last month especially towards the end of it when I swung back to the Left again. (Childe to Dutt, 6 October 1938 (1938a))

Childe was not unlettered in philosophy. He remarked at the beginning of *Society and Knowledge*: 'I have indeed read a lot of philosophical classics, mostly in the original, works like Plato's *Republic*, Aristotle's *Politics*, Kant's *Kritik*, Hegel's *Phänomenologie des Geistes*—and plenty of more recent works from Marx to Wiener' (Childe, 1956a: 1). So one can assume that his knowledge of Marx and Marxism was philosophical as much as political. By the early 1930s, at least, he was familiar with writings by Croce and Gentile (Green, 1981a: 83), who according to Jacoby (1981: 43–7) were, in their earlier works, Hegelian Marxists. Childe therefore was aware of a subjective tradition in some Marxist literature, which, to Jacoby, is one of the features of western Marxism. Indeed, in a letter to Dutt written eight days after the one quoted above, Childe wrote:

> just to venture into metaphysics, I do not believe that there are uniformities in history of the kind from which one can make such predictions as work in natural science. I believe

> that the historical process as a whole is the actual creation of genuinely new values and that is why I am neither a Christian or a Hegelian. I am not sure whether Marx got beyond Hegel in this; I suspect he did but his commentators have successfully missed the point. Of the neo-Hegelians Gentile alone has made it unconditionally plain. (Childe, 1938b)

These remarks could have provided the introductory text to his personal testament, 'My Beliefs', set out in *Society and Knowledge* (1956a: Ch. X).

Chided quite properly by Trigger (1984: 1) when I complained that the books by McNairn, Green and himself did not deal sufficiently with Childe's Marxism (Gathercole, 1982: 197), I have spent much time trying to understand Childe's attitude in this respect. Often have I been reminded of that grotesque character, Bagshaw, whose nickname, 'Books-do-furnish-a-room', provided the title of one of the novels in Anthony Powell's sequence, *A Dance to the Music of Time*. Journalist, literary hack, critic, editor—ultimately transformed into Lindsay Bagshaw, television personality—he 'was for ever fascinated by revolutionary techniques, always prepared to explain everybody's standpoint, who was a party-member, fellow-traveller, crypto, trotskyist, anarchist, anarcho-syndicalist, every refinement of marxist theory, every subtle distinction within groups' (Powell, 1972: 37). No wonder, to accept for a moment this catalogue of categories behind the satire, Daniel could say in 1958 that the great puzzle about Childe was the extent to which he was a Marxist (Daniel, 1958: 66). Moreover, while his British (and other) professional colleagues queried the existence of his Marxism, their Soviet equivalents and British Marxist intellectuals would often maintain that his books were not sufficiently Marxist (see below pp. 39–41). Does this confusion mean that Childe was perhaps a 'fellow-traveller', a 'quasi-', a 'mere' economic materialist, or even a phoney? In the tightly political, logic-chopping climate of his time such questions were fair to ask—and still are.

I think that Childe was a philosophical Marxist from his student days (cf. Green, 1981a: 18), for at least a decade before he obtained academic employment as Librarian at the Royal Anthropological Institute in 1925. In this respect Dutt's influence at Oxford was probably very significant (R.P. Dutt, 1957, 1965; Page Arnot, 1959, 1960; Gathercole, 1989: 85). But this does not mean that he endorsed Marxism–Leninism, especially as it evolved during Stalin's hegemony, nor that he regarded the Bolshevik revolution as the only way to achieve socialism. Irving's studies of Childe's Australian writings and activities have convinced him that Childe was against both a Bolshevik solution and a syndicalist state in Australia after World War I (Irving, 1988: esp. 73–5). He regards Childe, alone among political commentators at the time 'as the only advocate of the strategy of public ownership and workers' control to justify it in terms of Australian developments' (Irving, n.d.: 39). He even sees Childe as a 'precursor' of those writers 'who stress that the working class party "acts simultaneously as a party of representation and as a major political socialization and social control agent, mediating between nation and class" ' (Irving, 1988: 76, quoting Panitch, 1976: 236).

The point to stress is a historical one. It would not have occurred to most of Childe's archaeological colleagues, to whom twentieth-century Marxism meant identification with

the Russian Revolution and its aftermath, that he could have been attracted to Marxism as a political philosophy *before* the Revolution took place. This is quite possible, given that at Oxford between 1914 and 1917 he encountered, for instance, a range of views concerning the political relevance of Marxism, from that of Dutt (then a member of the Independent Labour Party, and from 1915 a convinced Marxist) to the Guild Socialism of G.D.H. Cole. Within the heady atmosphere then prevailing among the membership of the Oxford University Socialist Society, there would have been argued many paths to socialism. I should add that in my paper on Childe's early Marxism I tried very hard to identify him with orthodox Marxism, especially when examining his attitude in *How Labour Governs* to the International Workers of the World (Gathercole, 1989: 83–4). Although I appreciated that the implication in the book was that there could be a specifically Australian way to socialism, I was blinded by his criticisms of the opportunism of the Labor Party's leadership to assume that he too saw socialist advance as possible only by class conflict, leading to the victory of the working class.

Childe's attitude to European prehistory was analogous to his approach to politics. He was as much fascinated by capitalism as was Marx ('If Marx was capitalism's greatest critic, he was also its greatest admirer' (Jacoby, 1981: 25)). To study its origins, to determine within them its progressive elements as much as its negative ones, became over the years a major preoccupation. This was the ultimate message of *The Prehistory of European Society*. But it would be foolish to maintain that, having 'come to' Marx in philosophical and political terms early during World War I, he then devoted much of his professional career attempting to apply Marxism to archaeology, mechanically and uncritically. Nor should one see Childe's espousal of Marxist archaeology simply as a reactive process quickened by external political events. The letter to Dutt quoted at the beginning of this section was written shortly after the Munich crisis of September 1938. Earlier that month he had been at the meeting of the British Association for the Advancement of Science at Cambridge. There, in contrast to the attitude taken by such critics of the government as Haldane and Bernal, Childe had spoken in support of Chamberlain's appeasement policy (Crowther, 1970: 201). A few days later, however, he criticized that policy in a letter to *The New Statesmen and Nation* (Childe, 1938c). Political oscillation, however, did not deny his Marxist commitment. On 14 October he wrote to Dutt in the following terms concerning the latter's suggestion that he should join the proposed teaching Faculty at Marx House in London:

> The only practical effect would be to tie round my neck a label and I don't like labels, especially if they are liable to be misleading. This probably is. To me Marxism means effectively a way of approach to and a methodological device for the interpretation of historical and archaeological material and I accept it because and in so far as it *works*. To the average communist and anti-communist alike (and presumably it is only for their benefit that this piece of labelling is necessary), Marxism means a set of dogmas—the words of the master from which as among medieval schoolmen, one must deduce truths which the scientist hopes to infer from experiment and observation (archaeologists can do some experimenting and are always amassing fresh observations). I want to be a scientist; why should either group be deceived into thinking I am a schoolman? I want to

> get good Marxist ideas across to my colleagues and students and in that I have had some success, but they would not listen if I *began* as a Marxist (in Man Makes Himself the class struggle is disclosed as a deduction from an imposing looking array of facts). Were I competent I should like to get the same ideas over to the masses, but here too I feel one cuts more ice if one speaks as a prehistorian pure and simple. (Childe, 1938b)

Could one ask for a better definition of the role of the Marxist academic than this?

So *Man Makes Himself* has an importance concerning the development of Childe's thought irrespective of its status as a popular book. In setting out the argument for the significance of the neolithic and urban revolutions in the Near East, two landmarks in human progress which set the Old World in the mainstream of social development, Childe saw them as essential prerequisites for the eventual emergence of European, and then a wider, capitalism. Appropriately, it might be said, he wrote a Marxist work exposing the genesis of the system which, in the person of Marx, created the man, and so the doctrine, many would claim, earmarked to engineer its overthrow.

How early in his career was the genesis of Childe's archaeologically directed Marxism? Trigger has argued that this did not occur until the mid 1930s (Trigger, 1984: 4), following his visit to the USSR in 1935, when he made contact with Soviet archaeologists and obtained some of their publications. This, wrote Childe in 1949, was indeed what happened (Childe, 1949c: 251) although he did not indicate when. But it was not the whole story. He knew some of the Russian work already (cf. Childe, 1925a, 1935d). In addition, his two drafts on the origins of civilization for Bernal's Marxist History of Science (cf. p. 33) anticipate portions of the text that became *Man Makes Himself*, although they lack the latter's taut style, and the terms 'neolithic revolution' and 'urban revolution' do not appear. There is also earlier evidence that Childe was then thinking along Marxist lines. At the same time as he was preparing *New Light on the Most Ancient East* (1934a), he published in *The Plebs* an article entitled 'Where did Culture arise?':

> During that considerable period of 25,000 years man's bodily form, his hereditary physical make-up, has undergone only very minor modifications. On the other hand his culture...has been utterly transformed. The advance from a stone age savage to a western European is purely cultural. In studying that advance archaeologically we witness two major economic revolutions before that culminating in the 18th century.
>
> (Childe, 1933: 268, cf. 1934a: 283)

He then discussed the transition from food gathering to food production, followed by the change to 'men living together in large urban communities' (Childe, 1933: 268).

Tringham has argued for an even earlier manifestation of Marxism, in the sense that Childe 'experimented' with historical materialism in *The Most Ancient East* and *The Bronze Age*, works completed 'just before the earliest deliberate applications of historical materialism to archaeological data in the Soviet Union' (Tringham, 1983: 94). This interpretation is consistent with that argued above concerning the independence of Childe's Marxism and of his political theories concerning Australian socialism. On the other hand, Trigger's view is consistent with that taken by Childe himself, at least when he wrote 'Retrospect', where he

said that both *Man Makes Himself* and the 1939 edition of *The Dawn* fell short of Marxism (Childe, 1958a: 72). Was this a much later rationalization of the stance Childe had taken with Dutt in 1938, or, as seems more likely in the light of his post-1945 writings, did he consider that his perception of Marxism had changed since that time?

There is another way at looking at this matter, linked to the political environment within the USSR. By refusing to hitch his arguments to explicit Marxist–Leninist formulations, Childe was able to avoid a possible source of embarrassment to both himself and his Soviet colleagues in their personal relations. Trigger has pointed out that at this time Soviet scholars were not discussing the concept of the Asiatic Mode of Production (AMP), banned by the Communist Party of the Soviet Union (Bolsheviks) in 1931, 'the only form of class society to which Marx had not attributed inherent dynamism' (Trigger, 1984: 8, and discussion: 8–9). As far as I know, Childe did not use Marx's term directly, but he did use the latter's arguments concerning the limitations of Oriental despotisms. Sawer has shown, however, that Childe stressed 'a different aspect of Marx's analysis [namely]...the idea of the concentration of the surplus, rather than the idea of the organised nature of the economy, to explain the first flowering of civilization.' But this placed him, according to Sawer, in a position 'apparently counter to his general acceptance of the Marxist view of social dynamics. He asserted that it was the establishment of a *ruling class* in Oriental society that was responsible for the retardation of progress. According to Marxist historiography, the establishment of class divisions, and hence of a ruling class, was an essential concomitant of *progress* rather than a retarding factor' (Sawer, 1977: 61–2).

There is no reason to doubt the genuineness of Childe's position on the AMP, in opposition to Marxist–Leninist orthodoxy, and maybe this was one reason why he did not identify closely with the latter. He must have been aware of the extent of its dogmatic excesses, despite (or perhaps as shown by) the extent he played down their significance in print (Childe, 1935b, 1940a, 1941a; for the directly political aspect of his attitude, see below p. 42).

Presumably Soviet archaeologists knew of Childe's views concerning the AMP, if only because, shortly after World War II, they had knowledge of at least his popular books (he may have taken copies with him to the USSR when a member of a prestigious official delegation of scientists in the summer of 1945; but cf. Klejn, 1994: 78–9). *Man Makes Himself*, *What Happened in History* and *Progress and Archaeology* were reviewed together in the *Review of Ancient History* in 1947 by S. Ljaskovskij. This event was itself significant for Soviet historiography, because in Stalin's world the choice of books from the west reviewed in the Soviet academic press, and the nature of the review, carried messages for the colleagues of the reviewer. There were messages also, of course, though perhaps of a different kind, for the western colleagues of the person whose books were reviewed. In Childe's case the message for both groups was of guarded praise. It may have also been significant, at least for the Soviet readership, that chosen for review was almost the whole of Childe's popular output at that time.

Ljaskovskij considered that Childe, despite failings as a bourgeois scholar ('not a true Marxist' (ibid. 98)), was nonetheless in the progressive camp, and was sympathetic to

Soviet science. He complained, however, that in Childe's works there was no serious attempt to explain 'the "urban revolution" as a victory in the class struggle', and he failed 'to take account of the fundamental course of social development' (ibid. 101; cf. Sawer, 1977, above). The dead end of the AMP could hardly be construed as a class victory. The parallels with the contemporary situation within the USSR were obvious.

Ljaskovskij had other serious criticisms, for example, that 'Childe virtually omits the social mediation which Morgan, Marx and Engels throw into such bold relief' (1947: 100), and that 'despite the fact that he attempted to achieve a serious synthesis of primitive and ancient history his conception of historical development is confined to economic materialism. On the whole it is far removed from genuine Marxist ideology' (ibid. 104). Ljaskovskij's insistence that Childe 'has failed to understand the essential nature of class in ancient eastern society' (ibid. 103), and similar criticisms, give the impression of overall hostility, but many of Childe's detailed expositions were welcomed. Moreover, this was a long review of over six pages, indicating the importance accorded to his arguments. It is possible, therefore, that the unspoken message, alongside the explicit one of guarded approval, was that Childe's theoretical position had much to commend it.

Ljaskovskij's complaints persisted in one form or another in other Soviet criticism, not only in Mongajt's 'Crisis in Bourgeois Archaeology' (where the point was not pressed (Mongajt, 1951: 15)), but also in the introductions to Childe's three books translated into Russian: *Progress and Archaeology* (Arcikhovskij, 1949), *The Dawn* (Mongajt, 1952) and *New Light on the Most Ancient East* (Avdiev, 1956). As far as I know, he never responded to these criticisms except for a brief acknowledgement of Mongajt's comments, made, appropriately enough, in the Preface to the last edition of *The Dawn* (Childe, 1957: xiii).

Orthodox British Marxists took a similar line to that taken by Soviet archaeologists. There was often the attitude that Childe 'hadn't quite made it'. Reviewing *What Happened in History* in *Labour Monthly*, Benjamin Farrington coupled praise with insistence that it was not 'fully Marxist' (Farrington, 1943). But it was the authoritative tone of George Thomson, Professor of Greek at Birmingham University, and sometime member of the Executive Committee of the Communist Party, that carried orthodox conviction. The most revealing example of the Party's assessment of Childe's work was the reception given to *History* (1947), published in the *Past and Present* series, where Childe served alongside Farrington on the editorial board. This venture was one expression of the active intellectual life of the Communist Party after 1945, particularly in London. Though not a Party member, Childe had a recognized place in the network of intellectuals who participated; indeed, the Lawn Road Flats, with established left-wing connections, provided a convenient base.

History, in one sense, was its author's specific contribution to this communist movement. Another manifestation was the revival, in 1945, of the journal, *The Modern Quarterly*, which had enjoyed a brief, and promising, existence in 1938–9. Childe and Thomson were both on its editorial board, of which Dutt was chairman.

Such was the political and social background to the Communist Party's assessment of *History*. First there appeared a welcoming, mildly critical review by Thomson in *Labour Monthly* (Thomson, 1948). In 1949, *The Modern Quarterly* accorded it, unprecedentedly,

simultaneous reviews by Christopher Hill, Sam Lilley and George Thomson (Hill, 1949; Lilley, 1949; Thomson, 1949). There was considerable praise, as Marxists, from each, although both Hill and Thomson felt that Childe paid insufficient attention to the role of the class struggle. 'The greater part of the book,' wrote Thomson, 'is devoted to an examination of three different conceptions of historical order, the "theological", the "naturalistic", and the "scientific", corresponding to subjective idealism, mechanical materialism and Marxism. Childe recognises that they are all products of class society, and has many illuminating things to say about them, but nowhere does he analyse them in terms of their class basis' (Thomson, 1949: 267). A secondary criticism was that Childe was insufficiently prepared to allow a major role to ethnographic parallels (for example, using Gaelic folklore to illuminate aspects of European prehistory (ibid. 269). For his part, Hill maintained that the transition to a wholly rational historical order was 'the *necessary outcome* [my italic] of the victory of the proletariat in a world scale in the present dilemma—"revolution or paralysis" ' (Hill, 1949: 262). The last phrase was quoted from *History*, where Childe had been quite explicit that he did not consider the historical process to lead inevitably to a socialist, then communist, society:

> In Mesopotamia, Egypt and China theocratic despotism, relations of production appropriate to the productive forces of the Bronze Age, persisted into the Iron Age. They effectively fettered the exploitation of the new forces represented by iron with the result that technology also stagnated. The whole life of those societies stagnated too; the first two eventually perished altogether. From a Marxian analysis all that one can deduce is the dilemma—revolution or paralysis. History does not disclose an unfaltering march to a predetermined goal. (Childe, 1947: 73)

Here, of course, Childe could point to the well-known statement in the Communist Manifesto about class conflict ending 'either in a revolutionary re-constitution of society at large, or in the common ruin of the contending classes' (Marx and Engels, 1976: 482).

Essentially the issue of contention was infallibility—of both Marxism–Leninism and Soviet leadership in the outcome of the class struggle; the political and the academic here coming together in this Cold War context. But for Childe the issue had a longer history, well illustrated by his approach to Engels' famous work *The Origin of the Family, Private Property and the State*, for many years deemed the basic Marxist text for the study of primitive society and prehistory. Though aware of its continued relevance concerning some details (cf. Childe, 1944e: 4, 11, 17, 20, 22) he maintained a cautious approach to the book (e.g. 1951a: 9–11, 28). Behind this attitude lay an intriguing story of conflict with Dutt over its enduring significance for Marxist theory.

Disagreement arose between the two men following their contact in 1938, when Childe's membership of the Faculty at Marx House was mooted (see above pp. 37–8). At the same time Dutt asked him to write notes to accompany a new translation of *The Origin of the Family*. Initially his reaction was very enthusiastic (Childe, 1938b). He went to California the following summer (to teach at Berkeley) equipped with a copy of the German edition. Chivvied by Dutt concerning progress, however, Childe replied in devastating terms:

> My dear Raji, Yes I have Der Ursprung der Familie with me here and confess that reading it makes me pessimistic of evolving therefrom a really useful or reliable synthesis. I feel that Engels was so seriously handicapped by his ignorance of the sort of material produced by archaeological research in the last fifty years & of the more reliable ethnographic data collected in the same interval as well as the immense amount of economic data on ancient Greece & Rome, to say nothing of the Oriental empires, obtained from inscriptions & papyri that the elements of permanent value in his book are very small compared with its glaring shortcomings—for wh[ich] of course he was not to blame. It does not even illustrate well the MCH [Materialist Conception of History] as I understand it. (Childe, 1939b)

Dutt was aghast. Criticism was the last thing he wanted; Childe's 'remarks give the effect of a hostile non-Marxist criticism', he wrote to Douglas Garman, an editor at Lawrence and Wishart, the intended publisher (Dutt, 1939). Childe withdrew from the project. And yet in a sense he did not, because he reviewed the new translation when it appeared the following year, where his criticisms were far more measured than his private comments might have led Dutt to expect:

> As an essay in Marxian method, a model demonstration of how social and political institutions can be correlated with methods of production and economic stages the book is a classic... It would be a tragedy if it were mistaken for a canonical account of what actually happened in human history... The student who applies to the richer and more reliable data now available the method exemplified in the text will find that they can be elucidated thereby no less satisfactorily than the distorted fragments available to Engels, and that in its general outlines the resultant picture will not differ too radically from that sketched in Chapter IX. (Childe, 1940d)

Was Childe perhaps asking the reader to do what he told Dutt he could not do?

One must remember the date. In 1940 the Soviet Union was more a potential enemy than a potential ally. For Childe to criticize Engels too strongly might seem to align himself with those who reviled the Communist Party for its anti-war stance; his criticisms of Soviet archaeology (1940a) and Soviet science (Childe, 1941a: 7) at this time were carefully equivocal. But once the USSR entered the war in June 1941 he felt able to discuss Soviet archaeology less ambiguously (Childe, 1942a, 1942b, 1942c, 1942e, 1943, 1944b, 1944d).

Childe's criticisms of Engels's use of ethnography, to fill the gaps in interpretation created by the absence of archaeological evidence, is I think, the root cause of his own careful use of the same technique. He has been criticized often for this, usually without detailed understanding of his own attitude, methodologically speaking, or of his knowledge of anthropology. Criticisms have ranged from the crushing comment by Julian Steward concerning *Social Evolution* that he should 'familiarise himself with the facts of ethnology' (Steward, 1953: 241), to Grahame Clark's near despairing statement that Childe had no appreciation, let alone understanding, of the functionalism of Malinowski or Radcliffe Brown: 'There is no sign that Childe was to the slightest degree influenced in his approach to archaeological material by the seminal writings of these men whose *Argonauts of the*

Pacific [*sic*] and *Andaman Islanders* appeared in 1922, the year he returned to England' (Clark, 1976: 6).

It is clear, when one examines Childe's knowledge in detail, that the issue was not one of ignorance, but of how he perceived his own knowledge, and how it was to be used. Steward's comment is inappropriate (to say no more) given Childe's overall knowledge of anthropology (here embracing ethnology and social anthropology), clearly evident from his reviews of R.K. Merton's *Social Theory and Social Structure* (Childe, 1951b) and S.F. Nadel's *Foundations of Social Anthropology* (Childe, 1951c). It might also be mentioned that he gave introductory but highly informative lectures on social anthropology (albeit to archaeology students!) at the Institute of Archaeology during this time, which showed a good command of the literature. In fact, he was commended by Meyer Fortes, the distinguished social anthropologist, precisely because of his knowledge of the subject (Fortes, 1970: 19, n. 1).

The reason why Childe was not a whole-hearted supporter of functionalism was that he considered it to have a basic methodological weakness, at least in its Radcliffe Brown/ Malinowskian version; he thought it liable 'to lead to conservative results, to the justification of the status quo' (Childe, 1951b: 367; cf. Childe, 1946c: 247; Spriggs, 1977: 8–9).

Nonetheless, Clark's comment has some force, at least at the methodological level. Childe's measured and objectively oriented approach to ethnographic analogy was perhaps pitched too often at a high level of abstraction: 'A Marxist prehistorian will aim at deducing from the assemblage he calls a culture a detailed picture of a working economic organization that can be compared point for point with existing preliterate societies' (Childe, 1979: 95). One yearns to be allowed to give some more flesh and blood to the bones.

At the same time, this mention of abstraction does recall a comment by Piggott. He drew attention to Childe's 'inability to appreciate the nature of archaeological evidence in the field, and the processes in its recovery, recognition, and interpretation, which led in turn to a tendency to ignore the potential inequality in the reliability of evidence he employed in his works of synthesis' (Piggott, 1958a: 308).

CONCLUSION

Any society 'may be allowed its own logic' asserted Childe in *Social Worlds of Knowledge* (1949a: 18), which underlines the fact that in assessing him and his career we should be careful to recapitulate as far as possible the social and intellectual contexts involved. Especially should we weigh the extent to which meanings of words and concepts have changed in the intervening period, partly no doubt because of the use he made of them.

These are obvious points, yet to effect them may not be easy. It can be argued that archaeology has changed so much since Childe's death that today's assessment is irrelevant. What would be the yardstick? However, as Trigger showed so convincingly in his Childe Memorial Lecture twelve years ago, Childe's views on many aspects of the archaeology he knew still had significance in 1982 (Trigger, 1982). Trigger's lecture was a definitive rejoinder to those critics who, by citing passages from his works out of context, cast him as a

straw man with many contradictory voices. On the other hand, it is deceptively easy to fall into anachronistic attitudes. For instance, Lord Renfrew has explored, *inter alia,* 'this central theme of Childe as almost, but not quite, a processual archaeologist (in the modern sense)' (Renfrew, 1994: 123). But is there any other than a modern sense for this term? It is possible to find in Childe's corpus interpretations recalling processual thinking, but it could be confusing, to say the least, to align Childe to this, or any other, contemporary archaeological school (remembering, incidentally, that he created none).

How binding, anyway, is context to historiographic writing? The question is not solely rhetorical. Central to historical assessment of Childe is the degree of understanding one has of the world he inhabited, and of the ways in which his work was understood. The most useful acid test in this last respect is the response by archaeologists to his version of Marxism. Here one can only say that it was poorly understood. I have already referred to the Soviet reception of his popular books (see pp. 39–40), where his Marxism was deemed false because it did not accord with the tenets of Marxism–Leninism. Among his colleagues in this country, in Mallowan's words, 'few of us took his political ideas seriously' (1977: 234), including, one might add, in contrast to at least some British Marxists (e.g. C.P. Dutt, 1950), his studies of the sociology of knowledge. Daniel, for example, who had a great regard for Childe as a scholar, and, to judge from his *Memoirs,* much affection for him as a man, eventually maintained that he was 'far from' being a Marxist, 'although he tried this Marxist, psychic-unity-of-man approach to the past in *Scotland before the Scots'* (Daniel, 1981: 162, cf. 1958: 66–7, 1986: 414–16). It is interesting to notice, given that Marxism is one of the major intellectual movements of this century, that Daniel, devoted to Cambridge all his adult life, could write about his student days with hardly any reference to the activities of the Cambridge Left, then so conspicuous (Sloan, 1938). 'I was an apolitical young man' (Daniel, 1986: 56).

Piggott and Clark, however, did take Childe's Marxism seriously. The former, reviewing *Scotland before the Scots* in *Man* was far more critical than was usually the case with reviews of Childe's books in this country: 'this book may serve, unfortunately, to discredit prehistoric studies in the eyes of those who are not archaeologists but who value the liberal tradition in scholarship and are vigilant of any encroachment on the freedom of scientific thought by totalitarian ideologies' (1947: 32). So here Childe was identified with the very Marxism–Leninism which Soviet archaeologists complained that he lacked.

Clark could be equally critical. When Childe contributed a long account of his recent visit to the USSR to the first number of the *Proceedings of the Prehistoric Society*, of which Clark was editor (Childe, 1935c), Clark responded sharply in the next issue with 'The other side of the Picture' (Clark, 1936). Forty years later, Clark considered the influence of Marxism on Childe's middle years to have been 'inhibiting'. In the same paper he unfortunately confused Marxism with 'Marrism' , though the confusion was to a degree of Childe's own doing. It was the appeal of the 'Marrists', wrote Childe in his autobiographical 'Valediction', not of the Marxists, 'to "uniformities of social evolution"...[which]...completely failed to explain the differences between one culture and another and indeed obliterated or dismissed as irrelevant the differences observed' (Childe, 1958b: 6–7; Clark, 1976: 3;

cf. Spriggs, 1977: 5–7). But Childe had not made clear what Marrism was, nor why he had used the word in that context. The Marrists were identified as a group of intellectuals in the USSR hostile to orthodox Marxism–Leninism, but only after Stalin had intervened in a controversy raging from 1949 to 1950 concerning the significance, especially with reference to social evolution, of the theories of the linguist, N.Y. Marr (who had died in 1934). Paradoxically, one of the results of this controversy was to encourage a rather less dogmatic distinction in Marxist theory between base and superstructure. So Marrism became Childe's shorthand for Marxism–Leninism (cf. Childe, 1956c: 154; 1958a: 72–3).

So, the cynic might say, Childe played his political games to the end, although open political references in his archaeological writings are few, often oblique and sometimes resistant to discovery—and therefore important. But how does one explain his practice of publishing articles on the same or very similar subjects concurrently in academic and left-wing journals? Of the several examples I cite the appearance in *The Modern Quarterly* of 'Social Implications of the Three "Ages" ' (Childe, 1946b), which was a complementary discussion to 'Archaeological Ages as Technological Stages', the Huxley Memorial lecture of the Royal Anthropological Institute (Childe, 1944a). There was surely small chance of overlapping readerships in this instance, although the paper of 1944 did get a terse footnote in *The Modern Quarterly*. Perhaps the origin of this practice of writing for two often distinct audiences lay in the genesis of *Man Makes Himself*, discussed above, or even earlier in the articles in *The Plebs*, with their blend of educational and Marxist zeal. Later, one might see in the exigencies of the Cold War the tactical need to separate academic (non-political, 'objective') writing from specifically political statements. On the other hand, of course, Childe did write legitimately for each of these two audiences, more divorced from each other in his lifetime perhaps than they are today. And so one arrives at what might be seen as a tragedy in his life; that he lived in two worlds, for whatever reasons kept apart from each other, where he was liable to be misunderstood by those who inhabited only one of them. These worlds did come together during the Second World War, but then only briefly.

It is fitting, therefore, that the Institute intends to publish some of the hitherto unknown lectures on Soviet archaeology given by Childe in Edinburgh in 1942 (cf. Fig. 1). They carry a message beyond themselves, of support for rational enquiry across sectarian frontiers, which his Soviet colleagues must have welcomed, and which is still relevant today. But the historical Childe has an additional message for us who work across cultural frontiers, whether past or present, well expressed by Tim Champion: 'more than any other writer before or since, he faced the critical problems of how knowledge of the prehistoric past is possible, and what sort of knowledge it is' (Champion, 1981: 13).

In his essay on T.E. Lawrence, Christopher Caudwell referred to 'the Tolstoyan conception of the hero as a man who is really a slave to circumstances', but inverted the argument by maintaining that 'a hero dominates events only because he conforms closely with the law that produces them' (Caudwell, 1938: 23). I believe that Childe achieved greatness insofar as he acted in accord with the meaning and the spirit of Caudwell's aphorism.

Abstract

This paper builds on themes discussed in two other papers (Gathercole, 1989, Gathercole, forthcoming) concerning interconnections between various academic, political and personal aspects of Childe's life and career. It concentrates on three areas regarded as particularly illuminating in this respect: Childe's ability, as 'a man in a hurry' to capitalize on opportunities as they presented themselves, by transcending the demands of the immediate event for a more permanent longer term objective; his remarkable success as a popular writer, and (influencing his approach to all these activities) his perception and use of Marxism. It is argued that Childe's greatness lay in his ability to control events, in a very unstable world, because of his understanding of the factors producing them.

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The Great European Museum: The museum one cannot avoid and does not need to enter†

by KENNETH HUDSON*

Any building has two functions. The first is to keep some people out and to allow others in, and the second is to protect the people and objects inside it from damage and danger. It is both a sorting device and a shelter. The sorting process may depend on legal powers, so that only those with a right to enter are admitted, or on self-selection, which sifts out the people with no interest in what is inside. Such selection is not always perfect, just or foolproof. Many people might derive great pleasure from attending a performance of La Bohème at Covent Garden, but are unable to do so, because they do not have the money to buy the necessary ticket. Many others are inhibited from entering a church or Harrods or the National Gallery because they believe that they would find the atmosphere uncongenial. They are convinced that they would not belong. Lady Thatcher never travelled by train because she identified trains with her political enemies, with left-wing people. She did not lack the money with which to buy a ticket. She did not have the will to do so. I myself have never in my life attended a race-meeting, except once by accident on Newmarket Heath, or entered the premises of a golf club. In both cases the problem has not been a shortage of money or a police ban on my attendance, but a total lack of interest or curiosity.

But to walk along a street or go for a stroll in the country is quite another matter. One does not have to pay to do it, there are no conditions, the area is readily accessible to people of all tastes and every kind of social and educational background. No two people notice the same things in a street or from a boat on the river, unless they are confronted with something as dramatic as a road accident or a lost child crying. Nobody gives guidance or instructions as to what is considered to be particularly worthy of attention. The visit is unstructured in every way. A street is not plugged in to an educational department nor is it part of its raw material. Yet it is in every way a museum, a series of history lessons for anyone who has been provided with the clues needed to discover the information that is hidden in the building and along the roads and pavements.

Yet the idea of a street being a museum is still almost a revolutionary concept or at all events was until the birth of ecomuseums in the 1960s, when the walls of museums were broken down for the first time and Georges Henri Rivière and Hugues de Varine began to popularize and justify the thought that a complete area, with everything it contained, could

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be transformed into a museum. During the past two centuries we have become so used to thinking of museums as establishments that it is somewhat difficult to regard them in any other way. Most people would probably define a museum as a building containing a collection of historical objects or, going a little wider, a collection of yesterday's objects. An elephant shot and stuffed a year ago is a yesterday's object, but hardly an historical object. A painting painted last week is a yesterday's object. In that sense, of course, everything in a museum, including replicas and reconstructions is a yesterday's object. Its history begins when it is made and every replica is at the same time an original object. The distinction between originals and copies is purely academic. A copy can exercise the same intellectual function as an original, always providing that one does not know that it is a copy. Once the fact that it is a copy is revealed, emotional factors begin to operate and phrases like 'the magic of original objects' come into play. The interests of the students of Greek sculpture are perfectly well served by first-class replicas of the Parthenon marble friezes, but nothing less than the return of the originals to Athens will satisfy the patriotic feelings of the Greeks. A perfect copy of the Black Prince's armour or the shirt in which Charles I was beheaded would have little or no value to the person to whom only the original armour or the original shirt makes the heart beat faster.

So one task of the museum-in-a-building is to act as a special kind of shrine or bank vault, in which culturally precious things can, at least theoretically, be kept safe from moths, rust and thieves. This kind of protection demands a secure building of some kind, and it is customary for a museum to regard itself as a place in which objects can pass their days in safety. This security is, of course, relative. Some objects are safer than others, because they are worth less on the open market. Culturally something may be very valuable, but, because it is unlikely to fetch much if it should be put up for sale, there is little reason to steal it. It is the vanity of museums which puts their collections at risk. To put original paintings, for instance, on public display is both highly dangerous and foolish. From an educational point of view, first-class reproductions would do the job equally well and the originals could be kept in almost complete security in a strongroom, but, because tradition demands that the public shall be allowed to see the originals, a criminals' paradise is created, first by concentrating so many valuable objects within a small space and, second, by advertising the fact.

The problem does not, of course, affect only pictures. Jewellery, for instance, porcelain and precious metals are equally at risk. Yet in all these instances it is nearly always claimed that from the point of view of the general public there is more to be learnt from an original than from a copy. More accurately, it could perhaps be said that in certain cases the imagination is stirred more by an original than by a reproduction. Suppose, for instance, that one is shown the actual Bible which Milton used or the pipe smoked by Stanley Baldwin. By letting one's thoughts dwell on Milton reading this or that page of this particular Bible and on Baldwin stuffing tobacco into this particular pipe, one can certainly add to one's awareness of the past. But, in order for this to happen, one must already know quite a lot about the person concerned. To the person to whom Milton and Baldwin are complete strangers, the Bible is only a Bible and the pipe is only a pipe. The fact that both are originals adds nothing to the inner quality of the object, to its poetic value. Experts are another matter. For many, but not all purposes, they need access to originals.

I realise that to say that in most instances copies are just as useful as originals is a major heresy. The usual opinion, amounting to an article of faith, is that the business of a museum is to safeguard, show and interpret originals, not to display replicas and that the replica is essentially and inevitably inferior to the original. I can only say that in most cases I simply do not believe this to be true. A copy of a medieval pot has exactly the same effect on me as an original, assuming that it is a high-class copy. The same would be true of a reproduction of a Chippendale chair, an Armada cannon, a brontosaurus or a Roman chariot. I believe that, in seeing themselves primarily as collectors and conservers of original objects, museums neglect their prime function and exaggerate their own importance.

The chief function of a museum, in my opinion, is to give people a better understanding of the past and of the world around them, to enlighten them. Anything which they may do in addition to this is certainly not useless, but secondary. A natural history museum in Britain should be unhappy if three-quarters of its visitors were unable to identify six common British trees or distinguish a field of wheat from a field of maize, a rabbit from a hare or a mackerel from a herring. A motor museum should feel that it had failed if it did not make three-quarters of the people who tour its collections aware of why automobiles make such a powerful contribution to the pollution of the atmosphere. A museum of local history which did not undertake guided tours of the district should consider that it had failed in its duty. A museum exists in order to do, not only to be.

The basic question to be asked of any museum is whether it regards itself as a self-contained institution in its own right or as a centre of guidance for people who want to understand rather more of the world around them. The museum-as-an-institution, as a European, I call the Small European Museum; the world outside is the Great European Museum. They are, or should be, mutually interdependent. Each should recognize the existence and the importance of the other.

Let me give one or two examples of what I mean. During the summer of 1993 I happened to be contributing to a summer school at Dinard, on the Brittany coast. It is a pleasant, well-looked-after little town, which became popular during the Victorian period with upper and middle-class people from both France and England. When he was Prince of Wales, Edward VII often visited Dinard and enjoyed gambling at the Casino. It was a quiet, prosperous place, with appropriate hotels and amusements. Since the Prince of Wales bestowed his favours on it, it has had, like all seaside towns, to adapt itself to two World Wars, to the motorcar and to great social and economic changes. Yet it has never gone down-market, never failed to maintain its essentially superior character. How, one wonders, as one walks around the streets and enjoys the amenities, has it managed to achieve such an apparent miracle? Has it been blessed by a series of influential and far-seeing mayors? Is there something about its location, between the estuary and the coast, which exercised a certain discipline on the planners and the investors? Has the climate, not too hot and not too cold, been a factor in deciding what kind of holiday-makers will come and which kind will go elsewhere? Has all the riff-raff funnelled itself off towards the Mediterranean and the heat? Or has it been simply luck?

There is no local museum in Dinard which can help to answer these questions. The town

has a Museum of the Sea, but nothing else. In any case, museums everywhere concern themselves much more with the 'what and when' than with the 'how and why'. There is no easy way of discovering the stages by which Dinard grew, which streets and public buildings were built when and where the money came from. I have often thought that, instead of mere name-signs, every street should have an informative plaque, saying when it was built, extended and modified and what significant buildings it contains. This would encourage members of the public to think about the town or the village historically and, in many cases, to realise that the place where they lived was much more interesting than they thought. At present, very little of this kind of on-the-spot interpretation goes on, although in some countries, particularly France, it is not unusual to find, below the street name, a mini-biography of the notability who gave his name to the street or the square. But even to know that Charles Dickens was a writer and that he was born in 1812 and died in 1870 would be of some help in giving people their historical bearings.

Imagine how interesting it would be if every shop were encouraged or legally compelled to put up a list of the previous occupants near the entrance, with the dates of when they did business there and the trade they followed. One could hardly expect every householder to do the same—that is the task of a public library, where changes of occupancy are documented in street directories. It is, even so, curious and sad how few people seem to inform themselves in this relatively easy way about their predecessors at the same address as themselves. A great many, I have discovered, have no idea that street directories exist, nor that in many instances, the public library has a series that goes back to the eighteenth century.

Earlier this year (1993), I attended a conference at Utrecht, in the Netherlands, which was discussing the proposal to create what was called 'a museum quarter' in the city. By this was meant a relatively small area which contained the oldest buildings in the city and that also happened to include a number of museums. The 'museum quarter' was, therefore, both the oldest quarter and the quarter with museums. One could not object to this idea in principle, but I felt, and said, that it encouraged a very limited idea of what was and was not 'historical'. The growth or decline of a city, as of any inhabited place, is continuous and it is absurd to promote, say, fifteenth-century buildings as 'historic' and to relegate those belonging to the nineteenth century to obscurity. Anything built by man is potentially interesting and the task for those concerned with the Great Museum is to release the potential. The only way in which a house, shop or church dating to 1450 is more interesting than one that dates from 1850 or 1950 is that during its lifetime it has been used by more generations of people. Its age is not the most significant thing about it. What matters most is what successive generations have done with it and in it, and the nature of the adaptations which have been made in order to meet constantly changing social demands.

To perform such a task for the hundreds and thousands of buildings within its area is obviously beyond the capacity of any museum. What a museum can and should do, however, is to choose a single building, research its history in great detail and present the results in a way which explains to its visitors the method of identifying, layer by layer, the history of where they themselves live and work. The purpose of a museum is to stimulate curiosity, not to answer all possible questions. It should see itself primarily as a guide and leader.

In the course of a long life, I myself have lived in 14 houses, attended four schools and two universities, and been employed for periods of a year or longer in eight buildings or groups of buildings. Looking back, I am ashamed at how little I know, or ever knew, about the history of most of these premises. The single notable exception is the house in which I live at the present time and where I have been for forty years. The reason for this is that the house was new when I moved into it and nobody has ever lived in it but myself. The house occupied by my mother and father from the time when I was 10 until I was 18 was also new, but I have absolutely no information as to what has happened to it during the past sixty years. I have, to my great regret, never returned to it. For me, it died when I left it.

I am fairly sure that its present inhabitants are totally ignorant of certain very significant features of its construction, which have probably caused considerable problems during the past half-century. The house was half of several hundred identical pairs of semi-detached villas put up in the 1920s by a firm of speculative builders and sold for £850 each. In order to make a reasonable profit, every possible constructional corner had to be cut and the cheapest kinds of material had to be used. Once my family was installed in its new house, one of the earliest on the estate to be completed, it was possible to keep a daily watch on how the others were being built. For several years, I spent a lot of time observing builders at work and a number of the things that I saw remain fresh in my memory.

The subsoil in this part of the new London suburbs was heavy clay. The builders decided to put this clay to good use. They piled it into giant conical heaps, known as ballast heaps, up to 50 feet high, throwing coaldust over each layer and laying terracotta ventilation pipes radiating from the centre to the circumference as the mountain grew in height. When all this had been accomplished, they set fire to the heap—exactly how I was never able to discover. I suppose it was a trade secret. The heaps burnt slowly for weeks, producing a characteristic, but by no means unpleasant smell all over the district, and when all combustion had finally ceased the mass was left to cool and eventually pulled apart, to reveal tons of what looked like small pieces of red brick. This was afterwards used as aggregate for all the concrete needed on the estate, particularly in the foundations of the houses, instead of the more expensive gravel. It must have represented a saving of perhaps £25 on each house, a worthwhile amount. It had, however, one serious disadvantage. Its compressive strength was low, so that in time cracks began to appear in the walls of the houses, causing considerable repair bills.

Another little trick was to obtain large quantities of plaster from demolished buildings. This was ground up in rotating pug-mills and the resulting substance became the basis of the mortar on which the bricks were laid. It was a weak mortar, but its great advantage was that it cost virtually nothing. Portland cement was used only for pointing.

Over the years I have searched hard for any printed references to the ballast heap and the use of second-hand plaster. I have found nothing. Neither have any photographs turned up. What were well-known jerry-building practices at this time appear to be recorded only as items of folk memory. Yet techniques of this kind are an important part of building history. The great suburban expansion of the 1920s and 1930s depended on them. Without them, the houses would have cost more than potential purchasers would have been prepared to pay.

The financing and building of the new estates is a fascinating story and the houses and their romantic names are an important part of the Great European Museum.

Since the Second World War, a number of local museums have put on excellent exhibitions about the development of the new suburbs and these have undoubtedly stimulated the curiosity of the people who live in the houses concerned. But the amount of research required to construct a complete picture is so enormous that it can only be done by the people most directly concerned, the owners and tenants of the individual properties. All that museums can do is to establish Know Your House campaigns, and to help their progress by organizing illustrative exhibitions. A good example of the kind of enterprise that is helpful can be found at the Torekällberg Museum at Södertälje, south of Stockholm. This is an open-air museum, which contains 50 buildings moved from their original sites in the town and the surrounding rural areas. One of its attractions is a House of the Year scheme, in which every year one of the Museum's houses is carefully researched and its history and occupants illustrated in detail. The lesson to be learnt is 'You can do this for your own house'. The exhibitions are extremely popular with the public and there is evidence that some of the people who come to see them do in fact try to build up the same kind of story for the houses in which they themselves live.

The Torekällberg Museum emphasizes that its houses were formerly the homes of ordinary people, not of presidents, poets and industrialists. In this respect, its attitude is quite different from that of most of the museums and tourist agencies that organize city tours. They, understandably, tend to concentrate on what one might call the high spots, the outstanding pieces of architecture, the big buildings, the dwellings of the rich and famous. These, they have good reason to believe, are the things that tourists want to see, foreigners' items. The aim of Torekällberg is quite different, to interest local people in the places where they themselves live and to show them how to discover the information they need. The Great Södertälje Museum, like the Great West Bromwich Museum or the Great Monte Carlo Museum, thinks first of its residents, the people who are there all the time, and only very secondarily, if at all, of tourists. This is the essential difference between the museum and a Great Museum, although a museum of any kind which relies mainly on tourists for its visitors is very unlikely to rank among the most satisfactory or most satisfying museums, whatever its size. The natives and the locals should come first, and the museum should be created primarily for their benefit. Tourists should be regarded as a bonus. To produce something that is geared up for them is to create a rootless museum, an institution which, however brilliant it may be in itself, contributes little or nothing to the national culture. The Olympic Museum in Lausanne is such a museum. It is only accidentally in Lausanne and in Switzerland.

An important point for discussion in this connection is whether all types of museum are capable of feeding information into the Great Museum, of providing the clues which make it easier to interpret the wider world. Let us consider some of the possibilities and complicating factors. What contribution can an art gallery make, for instance? Obviously, it depends a great deal on what kind of art gallery it is and particularly on what sort of collection it has. The main purpose of any art gallery, one imagines, is to open the eyes of the public a little

wider and to increase their sensitivity to the world around them. What the artist saw should show the environment or some aspect of human life in a different way. This is what artists are for, the justification for their existence. Until Napoleon Bonaparte overran Europe and sent cartload after cartload of artistic loot back to Paris, it was normal for art galleries (such as there were), monasteries and churches to contain paintings and sculpture manufactured in that country, often specifically for the place where they were exhibited. If the artist himself belonged to the same country or, even better, the same region, the works of art belonged twice over to the national culture.

Napoleon, however, changed that situation for ever. By creating a pudding of paintings in the Louvre, with ingredients from all over Europe, he established a new tradition of seeing the art of several countries side by side in the same public building. Britain, Germany, Holland and the rest soon began to copy France and, in order to qualify for real importance, a gallery had to be able to offer the general public the opportunity to compare the works of artists from different countries and different periods, a privilege which had previously been limited to wealthy people who were able to travel widely.

Paradoxically, a great deal has been lost by giving people a wider choice. There is no longer the same pleasure of finding Italian art in Italy, Spanish art in Spain and Dutch art in Holland. Just as Italian food tastes better in the Italian environment, so Italian art has more flavour and a different flavour in Italy. But once restaurants in Rome and Milan offer exactly the same intercontinental diet that one can find nowadays in Hamburg or Dublin, and people in Italy dress in exactly the same way as people in the United States or Sweden, travel becomes pointless. One of the worst features of contemporary abstract art is its internationalism. It conveys no sense of place. Like Coca-Cola, it could have been made and bottled anywhere.

But this is possibly to paint too gloomy a picture of the situation. A great many art galleries do contain paintings and drawings of scenes and people within a particular district and these can certainly be said to widen and deepen the knowledge of local people of the place where they live. There is considerable value in the fact that the picture has been painted at all. If an artist has found this or that street or quarter or building interesting, it is automatically graded up in the public mind. Previously it may have been interesting, but now it is officially interesting. The concentration of television cameras on a place has the same effect of pulling what was previously uninteresting out of the shadows. So much of what art museums possess is not on public show that it is always worth asking the person in charge if there should happen to be a print, a drawing or even an oil painting showing the particular street or corner in which one is personally interested. And many local reference libraries, of course, have photographic collections that can serve the same purpose.

But the Great European Museum is not only about buildings. In the countryside, every field has a history and even in towns the story of a public park or a group of allotments can be very interesting. At Argenta, near Ferrara, in central Italy, the Marsh Museum was established three years ago in order to study and explain the historical ecology of the district, that is, the changes made by man over a period of many centuries to the appearance of this section of the Po delta. Since prehistoric times, the reclamation and settlement carried out by

successive civilizations has produced a constantly changing landscape and an unsurpassed source of archaeological and cultural information. The Museum is located in a former 'casino', once used by fishermen as a storehouse. This beautiful example of brick vernacular architecture is an historical exhibit in its own right, a memorial to a past culture. It has been sympathetically restored to make it suitable for use as a museum, as a centre for the exploration of the area and for research and teaching. The displays in the museum cover the vegetation, wildlife and related occupations of a large part of the Po delta, considered from an historical point of view. One of the excellent audio-visual presentations is based on the concept of ecological archaeology, the essence of which is that each stratum of ecological evidence is covered by another.

One useful way of investigating the Great Museum is to ask oneself, 'I wonder what would have been growing in this field fifty or a hundred years ago?' The results can be surprising. In the course of a recent car journey between Zagreb and Varazdin in Croatia, I happened to put this question to my companion concerning a large field of maize, as we drove by it. 'Oh,' he said, 'until the 1930s that was an oak forest belonging to the Princes of Thurn and Taxis, an aristocratic German family which once had a considerable amount of property in the Balkans. They found themselves short of money, so they had the oak trees cut down and sold for timber.' Subsequently, during the turmoil of the war and the post-war years, the Thurn and Taxis family pulled out of the area, or were thrown out, and their estate passed into new ownership. So, within sixty years, oaks had become maize and no doubt there are many more crop changes to come.

The Great European Museum cannot have a catalogue and its collections are too enormous for any kind of indexing to be practicable. Classification is both impossible and pointless. Any Great Museum is basically an attitude of mind, a wish to dig deeper and to discover all there is to know about familiar objects and customs, to push one's roots firmly into the ground and to understand one's links with the past. In this fascinating but never-ending search, the 25,000 museums of Europe are more or less well equipped and motivated to be one's guide.

Abstract

Every street, square, town, shop, field, house and village is in its special way a museum. It contains evidence of what man has done to it, generation by generation, and that evidence is available to everyone who sees it and walks around. It forms part of what one could call the Great London Museum, the Great Lacock Museum, the Great European Museum, or whatever other unit one chooses. But in order to understand what one is looking at, one has to be in possession of the necessary clues. It is part of the task of the institutions known as museums to provide those clues, to open the eyes of people to the world around us. Only a relatively small proportion of the population in any country ever enters a museum, but everyone has regular contact with some part of the Great Museum. The fundamental problem and opportunity is to get the average citizen interested in the story of what is familiar, and to cultivate the habit of historical curiosity.

Design structure and narrative in southern Scandinavian rock art

by CHRISTOPHER TILLEY*

INTRODUCTION

Prehistoric rock art constitutes one of the richest resources we have to hand to enable us to begin to read cosmologies and their relationship to the structuring of social relations. In this paper I want to try and provide a preliminary interpretation of a particular set of Bronze Age rock carvings from the west of Sweden concentrating on the carving site of Högsbyn in Älvsborgs county (Fig. 1). During a visit to the site two things struck me—the aesthetics of the very special landscape setting of the rock carvings by the northeastern shore of the Råvarpen lake and the obvious linearity in their arrangement along a rough south–north axis up a slope from the lake through pasture land to arable fields where the most northerly carved rocks are located. The initial proposition is that this linearity in the arrangement of the carvings provides a key element in attempting to understand them. Pursuing a general analogy with Palaeolithic cave art (Norbladh, personal communication) it might be suggested that the arrangement of types of carvings in the landscape is structured in a definite order. In cave art certain species of animals are usually located by the entrance, others in the main chambers and some deep down in the furthest recesses. Valleys with rock carvings might be imagined as being like vast open-air caves with particular designs and carving surfaces having a definite arrangement in this 'cave'. In a cave movement through space is obviously structured in that an individual moves from the entrance to the back of the cave passing a sequence of designs. Movement between rock carving surfaces might be similarly constrained although there is the obvious problem of not knowing where the 'entrance' is, where to begin the journey. In this paper I shall first attempt to demonstrate that the rock carvings at Högsbyn are sequenced in a definite order and then provide an interpretative understanding of the manner in which this order unfolds, enabling the construction of a narrative that makes sense of the arrangement of the carvings.

THE CARVING SITE

Rock carvings at Högsbyn have been known for at least 150 years but have only recently been recorded, documented and published (Svensson, 1982: 10–57). Svensson's excellent

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FIG. 1. The location of rock carving sites in Älvsborgs county, western Sweden. The site of Högsbyn is in the north of the overall distribution (after Svensson, 1982).

publication provides the basis for this research. In her volume Svensson provides illustrations of all but six of the 34 known carved rocks. Of those not illustrated in her book, only one had a single cupmark (rock number 2) and could not be located at the time of documentation (1981). The other five (rock numbers 8–12) were entirely overgrown and not recorded. Svensson does provide, however, some details about the content of the designs on these rocks from an earlier documentation of the site by Claesson, detailing what types of designs are present but not their frequencies or spatial relationships except for rocks 11 (a boat and a hand) and 12 (two cupmarks) (Svensson, 1982: 29). Rock 9 alone has cupmarks present on it; rock 8 shoe-soles and cupmarks. Only rock 10 has a more complex design structure which would be interesting to know about in more detail. Since these rocks are at least partially documented and all but one have a very simple set of designs, the analysis of spatial ordering in the designs which follows should not be unduly prejudiced.

Högsbyn has a minimum number of 1,100 individual motifs executed on 33 discrete rock areas, which for the purposes of the analysis were subdivided into 46 carving surfaces (Fig. 2). The following criteria were employed to define these carving surfaces:

(i) Large natural fissures demonstrably present at the time of carving demarcate a discrete carving surface on a single large rock area;
(ii) Viewing angle. Carving areas which are not intervisible on different slopes of the same rock (e.g. west; east) or carvings on a rock slope as opposed to those on the crown of the same rock;
(iii) Distance—large gaps of 2 m or more separating carving areas on the same rock surface.

I have followed Svensson's numbering system using lettering to define individual carving surfaces on the same rock, e.g. 25:A; 25:B etc.

DESIGN CLASSES

Twelve different design types can be distinguished (Fig. 3). Of these six are abstract or not representational and the others can be clearly identified as human beings or human body parts (hands and feet), animals, boats and shoe-soles. The frequency of these designs and their occurrence on individual carving surfaces is shown in Table 1.

Cupmarks, as in all rock carving areas in southern Scandinavia, are quantitatively dominant both in terms of numbers of motifs and carving surfaces on which they occur. Otherwise the abstract designs most common at Högsbyn are the circle-cross and wavy lines. Boats and shoe-soles are most frequent among the representational designs. The majority of rock carving surfaces possess representational designs (83%) in varying frequencies. Excluding the cupmarks and unidentifiable designs, 44% of the motifs are abstract in form.

DESIGN COMBINATIONS

In order to investigate the meanings of the designs, it is crucial to consider their relationship with each other, as part of the meaning of a design is likely to be constituted through its relationship with others. In this section I will try and attempt to determine whether there is

Key: 1 cupmark
2 circle-cross
3 circle
4 wavy lines
5 cross
6 infilled motif
7 humans
8 animals
9 feet
10 hands
11 boats
12 shoe-soles

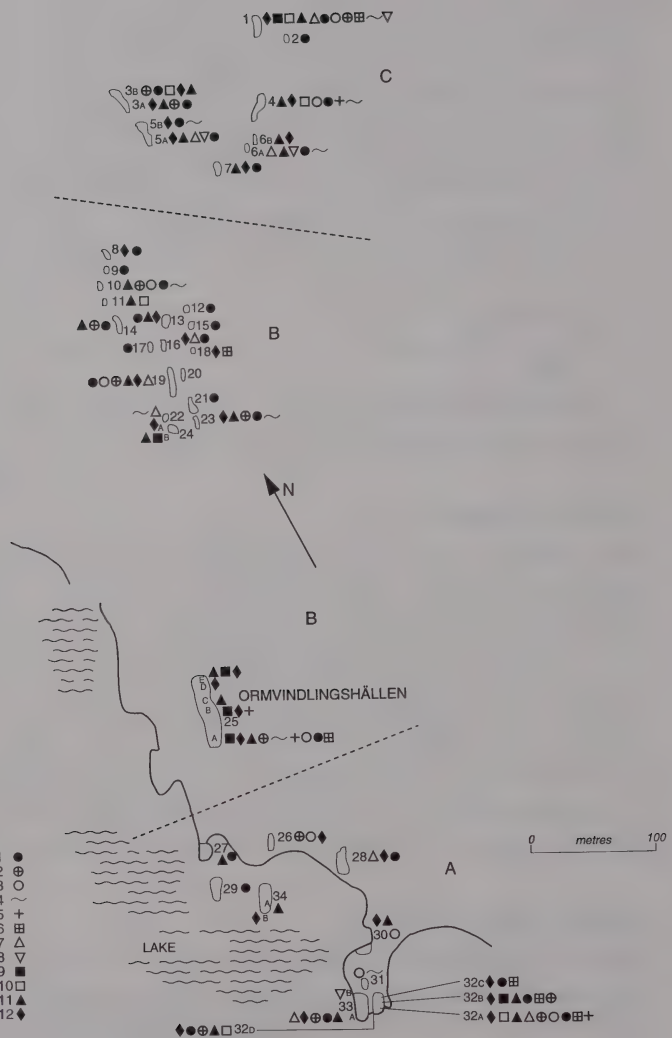

FIG. 2. The spatial distribution of rock carvings at Högsbyn showing the occurrence of different designs on individual carving surfaces. Carving surfaces on the same rock are differentiated by letters. A tripartite spatial zonation of the site from south to north (A, B, C) is shown.

TABLE 1. The motif types at Högsbyn in terms of number and relative frequency of carving surfaces on which they occur: total numbers of motifs and relative frequency (A%) and relative frequency excluding cup marks and unidentifiable designs (B%).

Design type	No. of carving surfaces	%	Frequency	A%	B%
cupmarks	29	63	537	49	-
circle-cross	13	28	41	4	10
circle	9	20	16	1	4
wavy lines	9	20	60	6	15
cross	3	6	7	0.5	2
infilled motifs	6	13	8	0.5	2
humans	8	17	29	3	7
animals	4	9	5	0.5	1
feet	6	13	25	3	7
hands	6	13	9	0.5	2
boats	24	52	69	7	19
shoe-soles	24	52	134	12	34
Unidentifiable	26	56	156		
Total	46		1096		

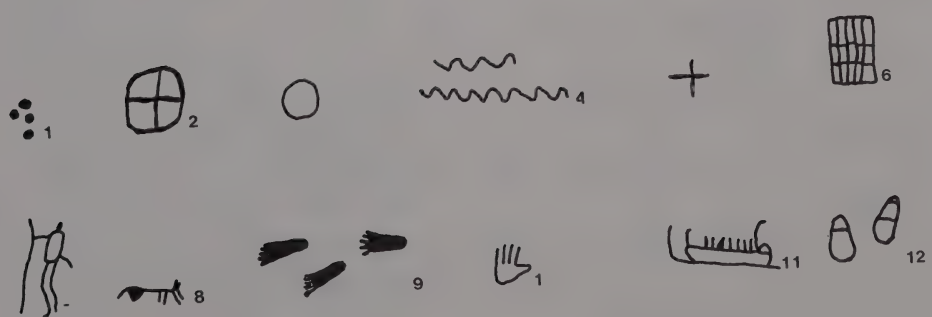

FIG. 3. The twelve major design classes present at Högsbyn.

any design grammar governing the relationships and associations of designs on individual carving surfaces, regarding each design as a unit of meaning in an overall structural and structured system, adopting a methodology discussed at length elsewhere (Tilley, 1991). The analysis is only concerned with identifiable free-standing designs rather than those forming parts of others (e.g. people in boats). The first stage in the analysis is to investigate which designs occur on their own on a carving surface and not associated with others and then consider the manner in which combinations of different design types are built up. At Högsbyn only 13 (28%) of the carving surfaces possess solely one design type (Table 2).

It is of great interest to note that only four of the twelve designs occur on their own. The others have a purely relational significance, i.e. part of their meaning is constituted by their association with other classes of designs. Carving surfaces with two different types of designs all possess unique combinations (Table 3) and their number is restricted. Out of 66 theoretically possible combinations only nine (14%) actually occur. Only three of the designs, the cross, animals and circle-crosses do not occur in these combinatorial pairs. As the first two design types are quantitatively infrequent this may not be significant but the absence of the circle-cross, occurring on 30% of all carving surfaces, is surprising. Two of these include humans and a further two body parts (hands or feet) in association with boats. Four of the combinations are made up of representational designs and a further four of representational and abstract designs. Only one purely non-representational design combination occurs.

A further ten carving surfaces have three different designs. The same design combination is only repeated twice. All other carving surfaces with three different designs are unique. This situation of non-repetition of design combinations is found on all carving surfaces with

TABLE 2. Carving surfaces with only one identifiable design type (e.g. one cupmark or a number of cupmarks).

Design type	No. of carving surfaces	%
cupmarks	7	15
shoe-soles	3	6
boats	2	4
animals	1	2

TABLE 3. The frequency of carving surfaces with two different identifiable designs.

Design combination	No. of carving surfaces
shoe-sole + human	1
boat + hand	1
boat + shoe-sole	1
boat + foot	1
shoe-sole + cupmark	1
boat + cupmark	1
human + wavy line	1
shoe-sole + infilled	1
circle + wavy line	1

TABLE 4. The frequency of design combinations on carving surfaces with three to eleven different designs.

Design combination	No. of carving surfaces
Three designs	
shoe-sole + cupmark + infilled	1
shoe-sole + cupmark + wavy line	1
shoe-sole + foot + cross	1
shoe-sole + boat + cupmark	2
shoe-sole + boat + circle	1
boat + circle-cross + cupmark	1
shoe-sole + human + cupmark	1
circle-cross + circle + shoe-sole	1
boat + shoe-sole + foot	1
Four designs	
boat + cupmark + circle-cross + shoe-sole	1
Five designs	
circle-cross + hand + cupmark + shoe-sole + boat	1
human + boat + animal + cupmark + wavy line	1
shoe-sole + boat + wavy lines + cupmark + circle-cross	1
boat + shoe-sole + circle-cross + cupmark + human	1
hand + boat + shoe-sole + circle-cross + cupmark	1
boat + shoe-sole + human + animal + cupmark	1
boat + circle-cross + cupmark + wavy lines	1
Six designs	
shoe-sole + foot + boat + circle-cross + cupmark + infilled design	1
human + shoe-sole + boat + circle-cross + circle + cupmark	1
Seven designs	
shoe-sole + boat + hand + cross + circle + cupmark + wavy lines	1
Nine designs	
foot + boat + shoe-sole + circle-cross + circle + infilled design + cross + cupmark + wavy line	1
shoe-sole + hand + boat + human + circle-cross + circle + cross + cupmark + infilled design	1
Eleven designs	
shoe-sole + foot + hand + boat + human + circle-cross + circle + cupmark + animal + infilled design + wavy lines	1

more than three designs (Table 4). On no carving surface do all design types occur together. The most complex surface has eleven design types, the cross not occurring. Surfaces with six or more different designs make up 13% of the rock carving area. The design grammar or permitted numbers of different combinations is extremely restricted. There is a lack of repetition such that beyond surfaces with only one design type, each surface, with only one exception (shoe-sole, boat and cupmark occurring twice), has a unique design combination. These combinations in all cases include both abstract and representational designs.

It is possible to investigate the structure, and sense, of this design grammar in more detail by considering the representational and abstract designs separately. Table 5 details combinations of representational designs.

TABLE 5. The frequency of carving surfaces with different combinations of identifiable representational designs.

Design combination	No. of carving surfaces
One design only	
shoe-soles	8
humans	1
animal	1
boat	5
hand	0
foot	0
Two designs	
shoe-sole + boat	6
shoe-sole + human	2
shoe-sole + hand	1
shoe-sole + foot	1
boat + hand	1
boat + foot	1
Three designs	
shoe-sole + boat + hand	2
shoe-sole + boat + foot	3
shoe-sole + boat + human	2
human + boat + animal	1
Four designs	
shoe-sole + human + hand + boat	1
shoe-sole + human + boat + animal	1
All designs	1

TABLE 6. The frequency of carving surfaces with different combinations of identifiable abstract designs.

Design combination	No. of carving surfaces
One design only	
cupmarks	18
cross	1
circle	1
wavy lines	1
infilled design	1
circle-cross	0
Two designs	
cupmark + circle-cross	5
cupmark + wavy line	2
cupmark + infilled design	1
circle-cross + circle	1
circle + wavy line	1
Three designs	
cupmark + circle-cross + wavy line	1
cupmark + infilled design + circle-cross	1
cupmark + circle + circle-cross	1
Four designs	
cupmark + wavy line + circle + cross	1
cupmark + circle + cross + circle-cross	1
Five designs	
circle-cross + circle + cross + cupmark + wavy line	1
All designs	1

Shoe-soles and boats, the most common representational designs, are the only ones to occur with any frequency on their own. They also are present in virtually all design combinations except when solely associated with feet or hands in pairs. Again the number of combinatorial possibilities is severely restricted and all representational designs occur together just once on one carving surface, rock surface 1 in the extreme north of the carving area. Turning to the abstract designs a similar situation occurs. The circle-cross never appears on its own and the only design to do so with any frequency is the cupmark—the most common design at Högsbyn. Combinatorial possibilities are restricted. All surfaces with more than two designs possess unique combinations and only one carving surface has all the abstract designs represented; surface 25:A in the middle of the carving area, the 'Ormvindlingshällen' (Table 6).

MERGING DESIGNS AND SUPERPOSITIONING

There are a considerable number of cases of design superpositioning and designs which join or merge into one another at Högsbyn (Table 7). It is assumed here that this was both intentional and constitutes part of the meaning of the designs through physical association.

Merging or overlap between identifiable designs occurs on 17 carving surfaces (37%). These surfaces occur throughout the carving area but are more frequent on the northern rocks in which merging or superpositioning includes a greater number of different individual designs. Spatially the nature of this overlap changes. On the southern rocks most cases involve boats, in the middle wavy lines, and in the north boats, wavy lines and shoe-

TABLE 7. Cases of design superpositioning or merging between different identifiable design classes.

		Design numbers											
No.	Design type	1	2	3	4	5	6	7	8	9	10	11	12
1	cupmarks		+	+	+							+	+
2	circle-cross	+			+							+	+
3	circle	+											+
4	wavy lines	+	+				+	+	+	+		+	+
5	cross												
6	infilled				+								+
7	humans				+							+	+
8	animals				+								
9	foot				+							+	+
10	hand												+
11	boat	+	+		+			+		+			+
12	shoe-sole	+	+	+	+		+	+		+	+	+	

soles. The latter merge or overlap with all other designs except the cross and animals, both of which are quantitatively insignificant. The shoe-soles may thus be claimed to articulate together other designs in an overall system of meaning.

MAKING SENSE OF HÖGSBYN: DESIGN FORM

Up to this point in the account a simple distinction between representational and abstract designs has been maintained and the crucial question of the meaning of design form has not been addressed. It is now necessary to ask: why these designs and what relationship is there between them? It is simplest to begin with the six designs that can clearly be identified as representing something in the world. These fall into three groups: (i) human beings and body parts of humans: feet and hands; (ii) animals; (iii) cultural objects: shoe-soles and boats.

Only five depictions of animals occur on four carving surfaces. The species in question are not readily identifiable. In two cases antlers appear to be depicted indicating the species as perhaps red deer. Two others with long tails and associated with human depictions may be dogs. Since these depictions are quantitatively so few they do not appear to be an important element in the Högsbyn carvings. Humans and body parts of humans never occur on their own on individual carving surfaces whereas shoe-soles and boats do, thus further serving to differentiate between these classes of designs and their significance.

Hands and feet only occur together with humans on two carving surfaces: that in the extreme south of the carving area, surface 32:A, and in the extreme north, surface 1 (see Fig. 2). On the southern surface they are separated from the human bodies while on the northern surface the humans occur in close relationship to the depictions of their body parts. Why are hands and feet detached from the human body as separate depictions and not other body parts?

A number of reasons can be suggested: (i) they are more or less identical extremities of the body; (ii) they move in a manner far more complex than the joints of the arms or legs; (iii) they are the primary means by which the human body is able to physically sustain itself. The feet carry the body through space; the hands actively permit the grasping and manipulation of the external world. The hands are the most expressive part of the physical body used to signal, command and complement verbal discourse. They are the primary medium through which humans work on, interact with, and transform the world. The rock carving surfaces themselves are, of course, transformed by the hand produced designs. Both the hands and the feet are in continuous contact with the external world beyond the body in a manner in which other body parts are not. Feet, carrying the body, represent its lowest point; hands raised upright on the arms are higher than any other part of the body.

Looking at the sequencing of hand and foot depictions from south to north through the Högsbyn carving area, an interesting pattern emerges. On the southernmost surface humans, hands and feet occur together. On rock 28 two stick-line humans occur together along with ambiguous feet/shoe-soles. In the middle zone of the rock carving area only feet occur on Ormvindlingshällen. On the next series of carved rocks depictions of humans and hands

occur on *separate* surfaces but feet are absent, only occurring together with hands and humans on rock 1. The human body, then, is being broken down and separated into its extremities, which are in turn separated, only to be associated again on rock 1:

South humans, foot, hand (spatially distant on carving surface)
Middle humans separated from feet
Middle humans separated from hands
North humans, foot, hand (spatially close together on carving surface)

A logic of similarity and difference thus connects together hands and feet. They are both to do with movement but while hands are high, feet on which the body walks are low. A number of properties connect hands and feet with boats and shoe-soles. Like hands and feet, boats and shoe-soles are intimately connected with movement: boats on the water, shoe-soles on the land. Shoes, worn on the feet, enable the body to move on land more efficiently; boats permit more efficient movement on water. They both enable the body to travel further and faster in time and space. But while shoe-soles are beneath the feet, boats ride high on the water. So a contrast between low and high links shoe-soles and feet, hands and boats:

Shoe-soles : Feet :: Boats : Hands :: Low : High

or shoe-soles are to feet as boats are to hands as low is to high. The above sequence may be re-arranged as follows:

boats (water) ⟶ shoe-soles ⟶ feet (land) ⟶ hands

LOW ⟶ HIGH

given that water is lower than land and that people stand in boats and on the land. High and low are, in turn, connected together with north and south as:

High : North :: Low : South

Such a linkage is suggested by a number of observations: the southern rock carvings by the lake are physically low. One moves up a slope towards the highest carvings of all which are in the north of the carving area. In the course of this movement the human depictions become larger, bolder, and 'higher' (see below). The largest and most elaborate boat depictions, reaching an overall length of over one metre, by contrast, occur on rock 32 in the south. The boats carved on carving surfaces in the rest of the Högsbyn area are generally smaller and less elaborate. It is only in the middle and north of the carving area that boats 'carry' and/or are superimposed on human figures, many of which are larger than the boats with which they are associated.

Three features may, then, be held to link together the representational designs:

(i) An intimate connection with the human body in *movement*;
(ii) A high/low contrast;
(iii) A south/north contrast in the character of the depictions.

Six 'abstract' design types make up the rest of the carvings at Högsbyn. As with the more obviously representational forms we can ask: why these designs? Is there any structural logic connecting them together and with the representational design classes?

Three of these designs, the circle, the cross and the circle-cross may be regarded as connected together inasmuch as a combination of the first two, as basic elements, is sufficient to constitute the third. The other three: cupmarks, wavy lines and infilled designs

are heterogeneous in character. The last category, in particular, has no real unity including a disparate set of forms (Fig. 4) and together with the cross is quantitatively insignificant. What does appear to link together the other five design forms is a concern with numeration, temporality, and the subdivision of space. I shall argue that they are intimately connected, like the representational designs, with movement—but in this case the movement of celestial bodies—and in particular the passage of the seasons.

On a number of carving surfaces the cupmarks are clearly arranged in sequences and multiples of six and twelve (Fig. 5). It is not hard to suggest that this may be related to lunar cycles or years given the common preoccupation with the passage of the seasons, solar and lunar observation in small-scale agricultural societies. Farming is quintessentially a seasonable task defined in a definite rhythm and correlated with the passage of the seasons. The agrarian calendar is an extremely important means of orchestrating group activity whose time is usually marked by ritual practices intimately connected with the sacred tasks of ploughing, sowing and reaping (Bourdieu, 1990). The circle-cross, long interpreted by archaeologists as a solar symbol or sun wheel, is divided into four segments. These segments may perhaps represent the summer and winter solstice and the spring and autumn equinox: fundamental natural markers of the passage of the year and the seasons (Fig. 6). This would explain why this design is so common at Högsbyn and throughout areas of southern Scandinavia with rock carvings. Otherwise it is particularly hard to account for its frequent occurrence and specific design form. A few of the circle-cross designs at Högsbyn are more complicated. The initial design form is further subdivided into eight, ten or sixteen segments. Some are extended in shape: longer or shorter seasons or periods of light? On one carving surface three circle-crosses are conjoined in a series suggesting years or different agricultural cycles (Fig. 7). Given the concern with numeration that has been demonstrated with the cupmarks it is of interest to note that a cupmark occurs only once within a circle-cross at Högsbyn on carving surface 1 which I have already noted has other distinctive characteristics. It marks the third quarter (autumn or harvest?) and this design occurs at more or less the centre of the carving surface (see Fig. 7 and Fig. 11). The wavy lines or 'snakes' can be understood in a similar manner as marking the passage of time, and perhaps in particular lunar motion. The moon, viewed from the earth, is the largest celestial body in the sky. It has a regular 28-day cycle waxing and waning, changing its shape, size and brightness every day. Lunar motion and its phases must have been noted to correlate with the seasons, in turn linked with agricultural activities. The moon's cycles involving growth, death and re-birth have always provided a metaphor for the life cycles of human existence. Brennan (1983) has plausibly interpreted the prevalence of the wavy line in Irish megalithic art as representing lunar cycles noting that:

> in many cultures the moon is symbolized by a snake or dragon because of its motion in the sky, weaving above and below the ecliptic (the sun's path) each month. During the year the moon meanders in a wavy line above and below the celestial equator.
>
> (Brennan, 1983: 137) (Fig. 8)

The wavy lines at Högsbyn may represent then days or months with each turn of the line

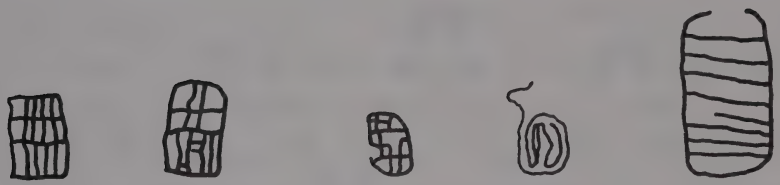

FIG. 4. Examples of infilled designs taken from different carving surfaces at Högsbyn.

FIG. 5. Examples of sequencing and a concern with temporality in relation to cupmarks and wavy lines.
1: These cupmarks (bottom row numbering 12) are the only carvings present on rock 17.
2: Double sequence of six cupmarks from surface 25:A (Ormvindlingshällen).
3: Cupmarks marking changes in the curves of the wavy lines on carving surface 25:A.

FIG. 6. The circle-cross as seasonal marker.

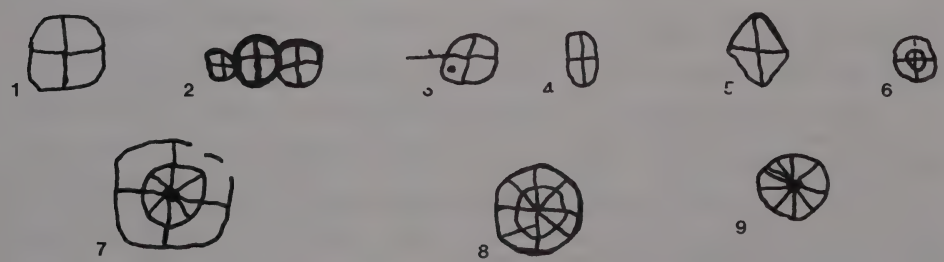

FIG. 7. Varieties of circle-cross designs taken from different rock carving surfaces at Högsbyn.

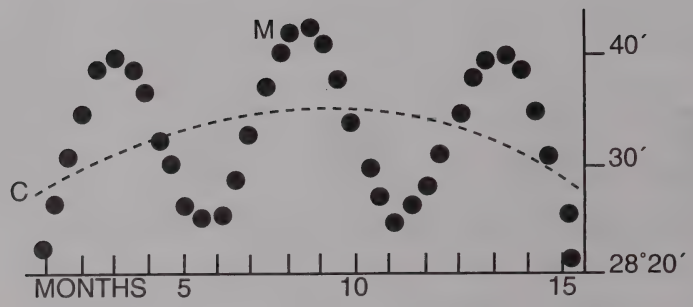

FIG. 8. The consecutive monthly maximum north and south declinations of the moon during a year. The path of the moon carries it in an ascending and descending motion above and below the celestial equator through the year (after Brennan, 1983).

representing a unit. Which unit is unclear as breaks in some of the wavy lines may be a result of weathering of the rock surface while in other cases it is likely that these lines were added to and extended over the years as part of a process of time reckoning. It is interesting to note that the main carving surface on which the wavy line occurs at Högsbyn has the largest number of circle-crosses (ten) and that changes in the wavy lines are sometimes marked with cupmarks which, as has already been noted, do occur in linear sequences of six and twelve. These wavy lines are also superimposed on, begin or terminate with shoe-soles (linked with movement) (Fig. 10). One of the wavy lines on surface 25:A begins or ends in an infilled figure that from its shape may represent the waxing and waning crescents of the moon. Out of this design the wavy lines 'emerge'.

The 'abstract' designs at Högsbyn can then be argued to be linked together as an expression of seasonal, solar and lunar cycles. In the most abstract sense the concern is with movement and change. And it is this concern with movement that logically links them together with the representational designs discussed above and the oppositions between high and low, south and north.

Looking at the entire rock carving areas there are two oppositional axes structuring the spatial organization of the most frequent designs. One axis is shared by boats and wavy lines, the other by shoe-soles, feet and humans. If boats and wavy lines are orientated in one direction, e.g. west to east along the rock surface, then invariably the humans, shoe-soles and feet are orientated north-south or vice versa. The particular orientation does not matter but the relative orientation does, i.e. that it should be reversed in terms of horizontal and vertical planes. If boats point in one direction, shoe-soles point in another. Some of the abstract designs, notably the cross, circle-cross and infilled types with a chequerboard pattern effectively combine both orientational axes on various carving surfaces. A spatial logic of orientation thus also links together and opposes particular design classes. Such a pattern can be taken further.

The relationship between the human depictions and the living is clearly denotative and metaphorical. While the human depictions stand for human beings in a direct manner, three other representational designs, hands, feet and shoe-soles are connotative signs with a metonymical significance—the part stands for the whole—they signify but substitute for humanity at a second order or level of meaning. This is reinforced by the fact that the human depictions, shoe-soles and feet are usually orientated on the same axis opposing the ships and wavy lines. Just as feet connote human beings in a relationship of metonymy it can be suggested that wavy lines on some carving surfaces either substitute for or supplement boats. The relationship in both cases is a transformative one: from humans to feet; from boats to wavy lines. Here the notion of motion can be re-introduced again. While shoe-soles and feet are obviously connected with normal human motion and activity on the land, the movement of boats on the water is of a different order and in a different medium. The close connection between the ships and the wavy lines suggests that the former may in fact be celestial signifiers rather than real boats and connected with celestial motion. Their significance goes far beyond being just something people ride in on the water. This connection of the boat with cosmology has long been pointed out by archaeologists. One suggestion has been that the boats may be conveying the dead to another world or are connected to agricultural rites. Marstrander, in particular, has noted that:

> the ship figures may well have been intended to represent fertility ceremonies in which the vessel of the god of vegetation was drawn ashore and across the fields in order to arouse the powers of growth and make the land yield good crops... In the folklore of both Scandinavia and western Europe there are traces of ceremonies in which ships were borne over fields in the spring as part of fruition rites. (Marstrander, 1963: 454)

TOWARDS A HERMENEUTICS OF THE CARVING SPACE AT HÖGSBYN

Since all the carving surfaces with the more complex design combinations are unique, and only two surfaces possess respectively all the abstract or all the representational designs, it is pertinent to examine their spatial distribution. The rock carving surfaces at Högsbyn are clearly arranged in a roughly linear series running from the most southerly carved rocks located in the Råvarpen lake and on the small Ronarudden that juts into it to those in the north situated in good arable land. From south to north the character of the rock carving surfaces changes substantially.

The entire rock carving area can be provisionally subdivided into three groupings of carved rock surfaces—those around and in the lake in the south, those in the middle situated today on pasture land and those in the north in arable fields (Fig. 2). The progression today is one from water to grazing land to arable land but during the Bronze Age the majority of the carvings were probably located in and around arable land. This close association between rock carvings and fertile agricultural land has been well documented by Bertilsson (1986) in relation to the Bohuslän carvings further to the west. To see all the carvings requires upward movement on a slope. The further the rocks are to the north in the sequence,

the higher they are. Looking at the overall frequency of the representational and abstract designs from south to north an interesting picture arises. Abstract designs, including or excluding cupmarks, are most frequent in the middle zone and least frequent in the south. By contrast numbers of representational designs increase from south (28%) to north (39%) and it is only the most northerly rock of all that has all the representational designs on it. Looking at design combinations, of the 14 carving surfaces in the south only 4 (28%) have combinations of more than four designs. In the middle zone, with 22 carved surfaces, again four (18%) have complex combinations while in the northernmost zone with ten surfaces, six (60%) have four or more different designs present (Table 8). Overall the trend is towards a greater complexity of design combinations in the north with the majority of the simplest carving surfaces occurring in the middle zone in which abstract designs are most frequent.

Figure 2 shows design combinations on the carving surfaces. The general picture to arise is a movement from more complex representational design combinations in the southern zone to simpler ones in the middle to more complex ones again in the north. No such trend is apparent for the abstract designs with more complex and simpler carving surfaces roughly evenly distributed through the three zones.

For individual classes of representational designs some interesting trends emerge. Animals are only found in the southern and northern zones. Only one hand occurs in the middle zone, the eight others in equal frequencies in the south and the north. Feet are most frequent

TABLE 8. The frequency of rock carving surfaces at Högsbyn with 1, 2, 3 etc. different designs according to a tripartite zonation (A, B, C) of the site (Fig. 2). Separate statistics are given for the six abstract and representational design classes and for both abstract and representational designs considered together.

	Abstract design			Representational designs			All designs		
Zone	A	B	C	A	B	C	A	B	C
1	4	11	3	6	8	1	4	8	1
2	4	1	4	2	6	3	2	5	1
3	2	2	-	3	3	2	4	5	2
4	-	1	1	1	-	1	-	-	2
5	1	-	1	-	-	-	2	2	2
6	-	1	-	-	-	1	1	1	-
7							-	-	1
8							-	-	-
9							1	1	-
10							-	-	-
11							-	-	1

in the middle zone (80%). The frequencies of human depictions and shoe-soles steadily increase from south to north. Frequencies of boats are virtually identical in the south and north but much lower in the middle zone (Table 9).

Generalizing from these statistics three distinct patterns emerge:

(i) Designs increasing in frequency from south to north (humans; shoe-soles).
(ii) Designs only common in the middle zone (feet).
(iii) Designs absent or infrequent in the middle zone but occurring in roughly equal frequencies in the south and north (boats; hands; animals).

There are clear trends apparent for three of the classes of abstract designs also. Circle-crosses steadily decrease in frequency from south to north. Only one wavy line is present in the south zone, 60% in the middle zone and the rest in the north. Cupmarks are again most frequent in the middle zone (47%), relatively common in the north, but few in the south (11%).

I can now begin to express some of these spatial relationships of representational and abstract designs in terms of a number of generalized generative principles:

(i) As shoe-soles and humans increase from south to north, circle-crosses decrease in frequency.
(ii) Where feet are common in the middle zone, so are cupmarks and wavy lines.
(iii) Where boats are common in the south and the north, hands and animals occur.
(iv) Where abstract designs dominate in the middle zone, carving surfaces with complex design combinations are more infrequent.

TABLE 9. The absolute and relative frequency of different designs according to a tripartite zonation (Fig. 2) of the Högsbyn carving site. Numbers for zone B include Ormvindlingshällen for which separate statistics are also given.

	Zone A		Zone B		Ormvindlingshällen		Zone C	
Design type	N	%	N	%	N	%	N	%
cupmarks	61	11	251	47	65	12	225	42
circle-cross	20	49	14	34	10	24	7	17
circle	5	31	7	43	5	31	4	25
wavy lines	1	2	34	56	29	48	25	42
cross	1	14	4	57	4	57	2	28
infilled	4	50	2	25	2	25	2	25
humans	7	24	9	31	-	-	13	45
animals	1	20	-	-	-	-	4	80
feet	1	4	20	80	20	80	4	16
hands	4	44	1	11	-	-	4	44
boats	27	39	17	25	4	6	25	36
shoe-soles	35	26	44	32	29	22	62	46

THREE ROCKS: A TALE OF THE RITUAL PROCESS AT HÖGSBYN

Excavations below two of the carved rocks at Högsbyn (Svensson, 1985) have revealed a stone platform in one case, at the foot of rock 19 (Fig. 2). This rock has a series of human depictions associated with boats and shoe-soles (Fig. 12). In the platform was a charcoal concentration and a deposition of quartz waste was placed immediately below the centre of the carving panel. Despite the radiocarbon dates (far later than the carvings) the stone platform and the quartz deposition may very well be connected with the ritual use of the carvings, especially in view of evidence from Østfold in southern Norway where platforms and depositions of smashed pottery are clearly associated with the carvings (Johansen, 1979).

The entire carving area of Högsbyn was almost certainly a focus for ceremonies involving processional movement between individual carving surfaces acting as a focus for specific ritual events. The designs occur on rocks in a 'consecrated' space whose meanings would be manipulated as part of a choreography of movement. I want to suggest specifically that the site was utilized as a symbolic resource forming an important focus for the ordering of calendrical and initiation rites. The carved surfaces, their inspection and interpretation, formed a kind of scene-setting for ritual events. We know that virtually all rites of passage and ritual events more generally occur through a tripartite organization of experience involving rites of separation from the profane world; the seclusion from this world in a liminal space; and rites of re-incorporation. In virtually all societies, including our own, individuals move through different socially recognized statuses as a series of discontinuous leaps. Each period takes various lengths of time with rituals marking the transitions: puberty rite, wedding, funeral, healing ritual etc. The hallmark of all these rituals is their social timelessness and the crossing of boundaries is always marked out by rites: a passage from x to not-x, not-y to y. These ceremonies have three purposes: (1) they proclaim a change of status; (2) they magically bring it about; (3) they are interval markers in social time.

A few basic and invariant elements of rites of passage may be summarized (Turner, 1967, 1969). The first phase involves a separation from normal life removing the initiate from normal existence who becomes abnormal in space, time, and appearance. The second phase is one of social timelessness, a liminal phase. The essential thing is that the initiate be removed from normal activities and kept physically apart—sent away or secluded. During this period of separation the initiate undergoes all kinds of prohibitions and proscriptions involving clothing, food or movement. For ordinary people the initiate is in a state of holiness: sacred, contaminated, dangerous, 'dirty'. During the liminal phase the characteristics of the ritual subject remain ambiguous. He or she passes through a cultural realm that has few of the characteristics of the past or the coming state. The third phase involves re-incorporation back into normal society in a new role; a rite of aggregation. These are often similar to rites of separation but in reverse, so for example processions may move in the opposite direction. The special costume or state of nakedness of the liminal state is removed along with food restrictions and a new costume appropriate to new status put on.

The second or liminal period is most crucial and may be emphasized in a wide variety of ways and always by contrast, for example: feasting/fasting; formality/ informality. Birth/

death symbolism is extremely common in marking out states. In initiation ceremonies involving the change from child to adult, bodily mutilation is frequent: circumcision; knocking out teeth; head shaving; scarification. These are all metaphors of purification and death. The child just dies for the adult to be reborn. The attributes of the liminal state are frequently ambiguous since this condition and the persons involved in it elude normal social classifications, statuses and positions. Liminality involves a process of *becoming*. It is betwixt and between; a kind of anti-structure or a reversal of the structure of day-to-day social existence and statuses.

In terms of the frequency of motifs three carved rocks stand out from all the others: rock 32 in the south with four carving surfaces, rock 25 with five surfaces and rock 1 in the far north. Together these three rocks possess roughly 50% of all the carvings, the most complex design combinations (Fig. 2) and a considerable degree of merging or superpositioning. Surface 25:A is the only one to possess all the abstract designs. Surface 1 has the greatest complexity of all design combinations and is the only rock to have all representational designs present on it. Surfaces 32:A and 25:A both have nine different designs; surface 1 eleven. I want to argue that these rocks, respectively, represent and encode the three crucial ritual phases of separation, liminality and re-aggregation.

Rock 32 (Fig. 9) is situated at the southernmost point of the entire carving area. Its eastern facing surface is covered with carvings and is clearly subdivided by two deep natural fissures. The carvings are not broken by these fissures which must therefore have been already present when the rock was carved (Svensson, 1982: 49). The carving technique is uniform over the entire rock surface and provides no indication of any great temporal distinction between the carving of individual designs. The western slope of the rock, not visible from the eastern side, has a smaller number of carved designs.

The three eastern carving surfaces are very different and have a clear sequential order from south to north. The following regularities may be noted:

(i) The frequency of representational designs decreases as do the number of different motifs on the same carving surfaces. Shoe-soles, boats, hands and humans all occur in the southernmost area (Fig. 9:A). Only shoe-soles, boats and one possible foot occur in the middle, while the only clearly identifiable representational design on the northernmost part of the rock is the shoe-sole (Fig. 9:C).

(ii) The frequency of cupmarks increases from surface A to C, as does the degree of merging and superpositioning. There is such a high degree of superpositioning of designs in area C that it is impossible to clearly distinguish any design type apart from shoe-soles.

In looking at the three surfaces we move from south to north, from clearly distinguishable representational and abstract designs to a tangled mass of carved lines, from structure and order to what might be termed anti-structure and disorder. Humans and body parts of humans (hands) are only present at the beginning of this sequence in which the boats are largest, most prominent and frequent. The circle-cross only appears in areas A and B and a very elaborate spider's web type form of it occurs in area B internally subdivided into 15 spaces, as opposed to the normal fourfold division. Elaborate infilled designs (some resembling

FIG. 9. The carvings of the three eastern surfaces (A, B, C) of rock 32. Scale: 1:25 (Svensson, 1982).

the form of shoe-soles or boats with crew strokes in area A) occur on all three carving surfaces, one being clearly set apart from the other carvings in area C rather than being integrated with the other designs as in areas A and B. This may represent a process of separation from the normal order involving the dissolution of representations of humanity.

Rock 25 (Ormvindlingshällen—the snake meander rock), in the southern part of the middle zone at Högsbyn is the only one in the carving area to be spatially separated from other carved rocks by a considerable distance, *c.* 140 m away from the nearest carved rocks to the north and *c.* 80 m from those to the south. Its very separation from other rocks suggests liminality as does the degree of abstraction of the designs and the fact that their positioning *reverses* normal design orientation relationships at Högsbyn. On rocks 32 and 1 the axis of the boat depictions runs roughly south to north, longitudinally across the rock surface. The human beings (on rock 1), shoe-soles and feet are, by contrast, orientated west to east. However on rock surface 25:A this is reversed. The shoe-soles and feet are orientated north-south opposing the wavy lines running on a west-east axis, which, as has already been mentioned, may be symbolic transformations or substitutes for boats.

The rock is 40 m long from south to north and 3 m wide. It can be subdivided into five carving surfaces (Fig. 10:A-E) along its north-south axis on the basis of large gaps (between 2 and 17 m) separating carved areas. The largest and most impressive of these is at the southern end. Rock 25 contrasts in an interesting manner with rock 32. As one moves from south to north the character of the designs changes from being predominantly abstract to entirely representational, from surface 25:A dominated by wavy lines, circle-crosses and cupmarks, to a series of panels with feet, boats and shoe-soles. Superpositioning occurs frequently on the large southern carving surface but is absent (with one exception) on the four surfaces to the north. In effect this reverses the sequence encountered on rock 32. We move from the abstract to the representational. Human beings here are entirely absent, only being represented metonymically by the feet and shoe-soles. Boats are also absent with one or two exceptions. The main feature of surface 25:A is the dominance of the snake-like wavy lines merging or superimposed on circle-crosses, shoe-soles and internally divided designs. These wavy lines, I have argued, suggest a concern with time, the seasons, solar and lunar cycles all of which are powerful symbolic referents in many rites of passage. The only other representational designs present are feet. The three representational designs present on surface 25:A, feet, shoe-soles and boats are precisely the only ones to occur on the other four carving surfaces on rock 25, from south to north:

foot and shoe-sole
boat with four clear human figures as crew
shoe-soles
shoe-soles, feet and boat.

The different designs are at first separated out and then conjoined at the end of the sequence. The feet on the final surface on rock 25 are all orientated to the north, i.e. in the direction of the next series of carved rocks.

Rock 1, in the far north of the Högsbyn carving area, combines together a series of abstract and representational designs in a similar manner to carving surface 32:A in the far

Fig. 10. The five rock carving surfaces on Ormvindlingshällen. Scale: 1:25 (after Svensson, 1982).

south (Fig. 11). It has a number of unique features. As already noted it possesses the greatest number of different designs and is the only carving surface to include all representational design classes. More depictions of humans occur here (at least seven) than on any other carving surface. Not only do more humans occur here than on any other surface, but six of the seven depictions cluster at the northern end of the carved area. These six humans are interspersed, overlap with, and merge with hands, feet, shoe-soles and a circle-cross. This group of human figures is bounded by a series of wavy lines separating it from a contrasting design field dominated by boats and cupmarks. This rock, the only one in which all the representational designs are brought together, is clearly a culminating (re-aggregational) terminal point in the sequence of carved rocks. That this is so is crucially confirmed by the character of the human depictions.

HUMAN BODIES AT HÖGSBYN

A minimum number of 29 human depictions occur at Högsbyn on eight carving surfaces scattered throughout the rock carving area. These change substantially along a south to north axis. On the three southern carving surfaces with humans only one or two depictions occur. They are all small, ranging in size from *c.* 10 to *c.* 25 cm in height. The figures on these rocks are all simple incised stick-line representations with no discernible thickening in the torso or elsewhere in the body. None of the bodies have sexual organs, carry tools or do anything. Humanity is simply present and certainly not a key element in the design structure of these rocks; one element amongst many that disappears just beyond the Ronarudden peninsula to be replaced by shoe-soles and feet on the carving surfaces of rock 25.

Depictions of humans occur again on four rocks in the middle and towards the north of the site. On one small stick-line figures appear but on the other three rocks the imagery is substantially different. The figures are larger (25–50 cm in height). They are more numerous with up to six figures appearing and are clearly the key element on the carving surfaces. The torso is now marked out in outline technique. Some have head-dresses and the majority carry tools or weapons, spears in at least three cases, and perhaps a bow in another. On two of the surfaces they are closely associated with boats, and animal representations (very rare at Högsbyn) appear.

The only other area on which humans occur is on rock 1, the most northerly. Here the representations are again very different. At least six large (*c.* 40–50 cm) figures are tightly clustered, interspersed with hands, feet and shoe-soles. These are the most substantial human figures at Högsbyn, being entirely scooped out of the rock surface. Two figures probably carry swords, others objects of a ceremonial nature. This is the only carving surface on which sexual identity is clearly marked: three or four figures have a phallus. On this rock a 'marriage scene' (see Yates, 1990) occurs between two figures who may both be phallic.

This sequence of changing human depictions (Fig. 12) can be understood as a process of becoming, a growing formation and development of the human body involving an increase in stature, frequency and substance. On the final rock sexual identity becomes established. The movement is from depictions with a minimal human identity to the creation of fully-

FIG. 11. The rock carvings on rock 1. Scale: 1:25 (Svensson, 1982).

fledged social actors. The human designs represent a process of the coming into being and objectification of the self; the self or individual as being formed through ritual discourse.

The narrative of the human depictions literally embodies the theme of becoming fully human in terms of roles, statuses and gender identity. The human body is something on which a definite identity is gradually inscribed, the neutral sexless and child-like bodies of the southern rocks are dissolved into the shoe-soles and feet of rock 25 to re-emerge carrying tools and weapons, finally to be locked in a sexual embrace. The body is initially present, 'dies' and is submerged in a liminal state before appearing again in a fundamentally different form. It is pertinent in this respect to note that rock 25 ends in the depiction of *twelve* feet with toes pointing north towards the rock surfaces on which humanity appears again. Three of these surfaces, including carving surface 1, have *six* human figures. The self and the body are being transactionally created through a ritual process establishing its social and sexual identity. A social body is being fashioned out of an 'organic' body appearing on the southern rocks as a product of ritual discourse: a sociopolitics of becoming.

It is interesting to note the difference in the *scale* of the designs. The hands, feet and shoe-soles are the same size as real hands etc. and of a standardized size. All other design types vary considerably in size from one carving surface to another and are not in the same sense 'realistic' depictions. Furthermore there is a consistent emphasis on pairing. These design classes rarely occur on their own. The metonymical depiction of disembodied extremities of human beings may perhaps be an expression of a social rather than an individual body—membership of a clan, lineage or group. If this is the case there is a constant emphasis on the mediation of the individual body by the social body and vice versa. If the body parts and shoe-soles signify a social body we may claim that they are to do with *structure* while the individual metaphorical human depictions are bound up with the *agency* and becoming of individuals through specific rites. This would explain why the human figures change while the design form and size of hands, feet and shoe-soles remains standardized throughout the site.

CONCLUSION

It has been argued there is a definite structural order linking together the carvings at Högsbyn through a south to north sequence. Högsbyn is, in part, a narrative about becoming human. The carvings encode a process by which social and sexual identity is established and simultaneously linked to the passage of time, fertility and the agrarian cycle. The art can be considered to be a public statement relating to formal within-group relations and social categories, structured in space to make specific sets of statements. The designs constitute multidimensional chains with shifting and transformational relationships between signifiers whose meaning becomes contextually fixed according to location in the series of carved rocks and their relationship to other signifiers on individual carving surfaces.

These carving surfaces formed a focus for processions and ritual ceremonies; the carvings themselves encoding ritual experiences and states that were actually acted out by people utilizing the site as a centre for initiation and calendrical rites. The meanings encoded in the carved rocks build up a complex narrative interpreted, used and re-used in ceremonies. The carved rocks formed a theatrical focus for schemes of perception, appreciation and action in

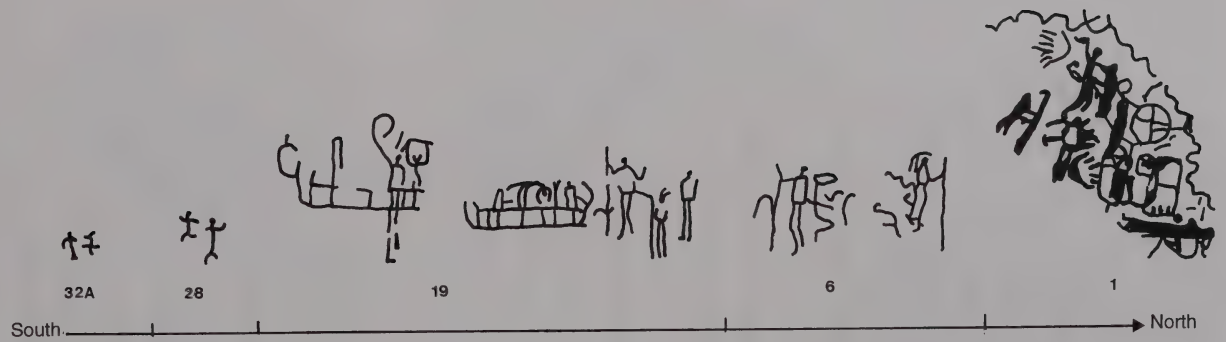

Fig. 12. Changes in character of the human depictions from the south to the north of the Högsbyn carving area. Scale 1:25. Numbers refer to carving surfaces marked on Fig. 1.

which the meaning itself was polythetic, i.e. it was built up through looking not at one rock *after* another but *step by step*: one carving surface and its meanings supplemented and modified by the next and so on.

Abstract

This paper examines in detail the spatial patterning of Bronze Age rock carvings from the site of Högsbyn, western Sweden. Combinatorial relationships between different classes of designs are examined on individual carving surfaces and the potential meanings of changes in design form across the carving area are investigated. It is argued that particular classes of designs, such as the human depictions, are arranged so as to construct a visual 'narrative' sequence as people moved around and between the carved rock surfaces, and that this formed an integral part of ceremonial practices through which individual and social identities became established.

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Routes of passage: Later prehistoric settlement and exploitation of a frontier region in northwestern Pakistan

by K.D. THOMAS* and J.R. KNOX**

INTRODUCTION

The rise and fall of the Harappan, or Indus Valley, civilization has long been a principal area of interest for prehistorians of South Asia. The origins of this civilization were once sought outside the Greater Indus region, principally through influences from Mesopotamia, and its decline has been 'explained' in terms of outside influences, e.g. invasions by 'Aryans' from Central Asia. Neither of these hypotheses now holds sway in explaining either the origins or the decline of the Harappan civilization; processes internal to the region are now favoured. The Harappans were not, however, at any time in their development isolated from the world outside the Greater Indus system. They had well-established trade or exchange networks. Trade routes with Iran, Mesopotamia and the Gulf, by land along the Makran coast or by sea, have been particularly emphasized (e.g. Ratnagar, 1981), although Chakrabarti (1990: chap. 3) has also discussed Harappan trade with Afghanistan and Central Asia through the northern and western mountain passes. Recent advances in our knowledge of the Bronze Age Oxus civilization in Central Asia (Lamberg-Karlovsky, 1994), coupled with the occurrence of Harappan levels at the site of Shortugai on the Oxus river (Francfort, 1984), should help to provide a more balanced view of the trade networks of the Harappan civilization.

Inter-regional contacts, even if not *trade* in the formal sense, with Central Asia are also probable for earlier periods, such as the Early Harappan (including Kot Dijian) and Pre-Harappan, e.g. Rehman Dheri I (Durrani, 1988), cultures (Chakrabarti, 1990: 129–30). Passes through montainous terrain have been vital as routes of passage for peoples engaged in trade or migrations, and many ancient settlements in the north-west of Pakistan are located near to such passes.

These passes, and the hills and mountains through which they cut, do not only inform us about large-scale patterns of trade and the migrations of peoples and cultures: the archaeology (and anthropology) of natural frontiers, such as mountain barriers, is of great interest in its own right for the study of human adaptations to such environments, and of the interrelationships between human populations on either side of them. The North-West Frontier of

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PLATE 1. View across the Tochi river as it flows through a narrow pass through the hills, some 7 km upstream from the site of Tarakai Qila (Fig. 3, site 3).

PLATE 2. View upstream from near where Plate 1 was taken, showing the pass opening up into the Khajuri Plain, the first major stage for travellers up the Tochi pass. Date palms are clearly visible, and give the Khajuri Plain its name (*khajur* = dates).

the Indian Sub-continent is, perhaps, one of the best known (almost legendary) of such frontier zones.

In this paper we consider aspects of the occupation and exploitation of the frontier zone adjacent to Bannu Division[1] in the North-West Frontier Province of Pakistan. We do this by defining the characteristics of the frontier, considering how it was exploited in the historic past, and evaluating the evidence for its exploitation in later prehistoric (late Neolithic and Chalcolithic) periods.

Mountains, hills and passes

Bannu Division occupies a low topographical basin (about 150 m a.s.l.) to the west of the Indus river (Fig. 1). It is surrounded on all sides by hills and access to the basin is given by a number of passes through these hills (Fig. 2). The area is, therefore, of considerable interest in having quite discrete points of entry and exit, some of which lead to Afghanistan and, ultimately, to Central Asia, and others to the plains of the Indus, the Punjab, and the rest of India.

To the west of the floodplain of the Indus river, between Dera Ismail Khan and Peshawar, the Waziristan Hills (which rise to between 1,500 and 3,000 m) give way to the mountains of the Safed Koh Ranges. These rise to an average height of 3,600 m (Sikeram, the highest peak, reaches 4,760 m) and are usually covered in snow. Together, the hills of Waziristan

FIG. 1. Location of Bannu Division (in black) in Pakistan.

FIG. 2. Geomorphological map of the Bannu basin. The various passes mentioned in the text are indicated on this map, as follows:
A: Kurram; B: Tochi; C: Khaisor; D: Shaktu; E: passes to the Kohat plain; F: Darrah Tang; G: Pezu Kotal.

and the Safed Koh Ranges form a major physiographic barrier between Pakistan and Afghanistan. This barrier of hills and mountains is cut by rivers which have created four major passes, two of which lie outside the Bannu area. To the north is the Khyber Pass, perhaps the best known and, for the last few centuries, the most important. It allows traffic between Peshawar, Jalalabad and Kabul. South of the Bannu area is the Gomal Pass, allowing traffic between Kandahar in Afghanistan and Dera Ismail Khan and the plains of the Derajat in Pakistan. Within Bannu itself there are two major passes formed by the Kurram and Tochi rivers. All of these passes have permitted trade links with Afghanistan and Central Asia, as well as providing routes for military invasions, since at least Early Historic times—i.e. from the middle of the first century BC (Verma, 1978).

In the Bannu area the passes formed by the perennial Tochi and Kurram rivers are important routes for migration, trade and invasion. In historical times, and almost certainly earlier, the Kurram River, known as the Kurma'h and sometimes the Bangash by ancient authors (Raverty, 1878), was a major route between Bannu and Ghazni (and thence to Kabul). The Kurram route is a viable alternative to the Khyber; indeed, the route over the Paiwar Pass (blocked with snow in the winter) to the Kurram Pass was the shortest route between Kabul and India (Verma, 1978: 270). Both Chingiz Khan and Timur (Tamerlane) used the Kurram route for their forays into and out of India; indeed, the Kurram route seems to have been the most popular invasion route to India in pre-Mughal times. The Tochi Pass figures less frequently in history than the Kurram, but Babur once used it on a journey from Bannu[2] to Kabul in AD 1505, travelling via Ghazni (ibid. 104–5). The Kurram route was probably favoured because of better grazing grounds *en route* (Markham, 1879: 49).

In addition to these two major passes in Bannu, Markham (ibid.) has counted some thirty-two other passes through the Waziristan Hills, all cut by ephemeral torrents of varying size and seasonal longevity which dissipate themselves in the Bannu plain. Among these torrents, the Khaisor and Shaktu rivers (or *algads*, as such seasonal torrents are known in the local Waziri Pashtu) are the largest and most significant (Fig. 3). Raverty (1878: 472–3) gives quite a lot of detail about the Khaisor and Shaktu passes and the terrain along them, as well as discussing the correct nomenclature for them. Raverty takes issue with earlier British map makers who use different names for the torrents and passes in this area. According to Raverty, when it enters the Bannu plain the Khaisor should be known as the Gambila (which eventually unites with the Tochi, retaining the name of Gambila), but before that it should be called the Kewa. The pass through which the Khaisor (Raverty's Kewa) flows is the Kewa Tangaey (ibid. 471). Similarly, the pass cut by the Shaktu (Raverty's Kus'hto) is the Kus'hto Tangaey (ibid. 472). Our observations of current local usage suggest that the terms Khaisor and Shaktu are now widely used for these ephemeral streams, and that these names should therefore also be used for the passes (or *tangai*) that they have cut.

Alongside detailed nomenclatural discussion, Raverty provides topographical information about the two passes cut by the Khaisor and Shaktu torrents. The route up the Khaisor is not difficult and is followed by some of the Waziri clans in their migrations; 'in the bed of the river they cultivate the available plots of alluvial land, here called *kats*' (ibid. 472). The route up the Shaktu is reckoned by Raverty to be more favourable for human migrations, and more important; at times it is separated from the pass of the Khaisor only by a range of flat-

topped hills, which are wooded on their slopes and grassy on their tops (providing pasture for flocks). The course up the pass is quite good, opening out in places to afford areas of alluvial land for cultivation or grazing. In some places the pass becomes a narrow gorge, which can be difficult to traverse when the Shaktu is in spate in early summer following snow melt in the mountains and higher hills. Ultimately the pass leads to the Waziri towns of Razmak and Makin.

As noted above, Bannu Division is completely surrounded by hills and is only accessible through various passes. Numerous ephemeral streams emerge from passes cut through the northern hills (Fig. 2), some of which afford access to the Kohat Plateau, and hence the Kohat Pass and the Vale of Peshawar. To the east of the Bannu basin the hills are cut by the Kurram at Darrah Tang, the river flowing on eastwards to merge with the Indus. To the south the hills are cut by the Pezu *Kotal* (pass) (Fig. 2), giving access to the Gomal Plain and the modern city of Dera Ismail Khan.

Bannu, therefore, provides archaeologists with an interesting opportunity to study movements of people in the past. By focusing field surveys in the vicinities of the various passes, and distinguishing between permanent and transitory occupations of various periods, it should be possible to make inferences about the role of the frontier zone in influencing settlement patterns, and the major routes connecting the Bannu frontier zone with other areas.

Characteristics of the Bannu frontier zone

So far we have considered the North-West Frontier as a purely physical barrier which can only be traversed by a number of passes. There are, however, many other factors which must be considered for an understanding of the past human exploitation of this frontier zone.

One important question is: should such a barrier as the hills and mountains described above be regarded as a 'frontier', in the sense that it separates different, and potentially antagonistic, socio-political entities? In the days of the British Raj the Frontier certainly had this function: the Durand Line marked the boundary between Afghanistan and British India. The Frontier was both a line which must be held in the face of a perceived threat to British India from an expansionist Russia, and a conflict zone both between British India and Afghanistan, and between British rule and the desire of the various tribes living on the Frontier to be free of outside interference in their affairs. For the tribal inhabitants the Frontier hardly existed except as an area within which they might come into conflict with the British. Otherwise they moved freely across the Durand Line between Afghanistan and British India.

Before British rule came to the Frontier it was under the governance of the Sikhs, who had extended their rule northwards from the Punjab into Afghanistan. Earlier still, the present Frontier was part of the Kingdom of Kabul. In both cases the frontier did not have the same political significance that it acquired in later years, but the logistical problems arising from the presence of a major physical barrier, making communications vulnerable across the ruled territory, must have been significant and potentially destabilising.

Frontiers should not be defined solely in relation to the populations living on either side of them; people also live *on* them. In the case of the North-West Frontier this is especially

important: the Frontier has been occupied for centuries (and probably millennia) by semi-nomadic pastoral tribes who are fiercely independent. Such tribes have always been predatory on the settled agricultural communities occupying lands adjacent to the frontier zone. Many tribes control access to some, or most, of the passes through the hills and mountains, exacting tolls from travellers and traders—and even plundering them.

Finally, there are environmental and ecological factors which characterize many frontier zones, and these are especially important for understanding the North-West Frontier, both in terms of the patterns of human occupation of it and of human movements through it. The mountains and hills have, of course, their own climatic characteristics which relate directly to altitude. Some of the high peaks have snow cover almost all year round. Others, including the high passes, are covered (or blocked) with snow only in the winter. Summers are cool compared with the hot plains of Bannu. The mountains also constitute a major climatic barrier between the north-west of the Indian Sub-continent and Afghanistan. Thomas (1983a, 1983b and especially, 1986a) has reviewed the evidence for recent and past climates in the Bannu region. The Bannu region is in a climatic tension zone between Mediterranean-type climatic systems (with wet winters and dry summers) to the west and monsoonal climatic systems (with wet summers and dry winters) to the east. The Bannu region suffers from this tension: it usually receives little rain from either competing climatic system. The most *effective* rain, for agricultural purposes, in Bannu falls in the winter and early spring, when evapo-transpiration rates are relatively low. High evaporation rates in summer mean that, without irrigation, agriculture is virtually impossible. In addition, the productivity of rangelands is low in the hot summer, with the result that flocks of grazing and browsing animals have to be herded towards productive and predictable pastures. Many of these are in the hills and mountains of the Frontier region. Transhumant pastoralism is, therefore, another aspect of human adaptation to the 'frontier zone'.

The frontier zone also has quite diverse units of natural vegetation which affect the types of browse and pasture available to herded domestic animals. The vegetation in this area is largely determined by climate, and therefore shows marked zonation in relation to altitude, although it should be noted that the present-day vegetation throughout the area under discussion has been drastically modifed by human activities, and especially by overgrazing by herd animals. The vegetation of the Bannu plain falls into the category of tropical thorn forest; it is dominated by shrubs and trees such as *Tamarix*, *Acacia*, *Zizyphus*, *Capparis* and *Salvadora*. Tropical thorn forest also occurs on the low hills around the Bannu basin, with *Acacia modesta*, *Zizyphus* and *Capparis* being the most common taxa. This type of plant association grades upwards into sub-tropical semi-evergreen forest, with *Acacia modesta*, *Olea ferruginea* and *Monotheca buxifolia*. This in turn merges upwards (above 1500 m) into more temperate plant associations, especially the dry temperate evergreen oak forests dominated by low-growing *Quercus baloot* (= *Q. ilex*). At higher altitudes, especially near Wana, and Parachinar in the Kurram, the forests become increasingly dominated by pines, and near Parachinar there are alpine pastures above the pine forests. This ecological zonation will be returned to later in the paper, when we consider the evidence for prehistoric exploitation of the hill and montane zones of the Frontier.

Patterns of human exploitation of the frontier zone

The main categories of exploitation of the frontier zone, based on ethnographic and ethnohistorical studies (e.g. Raverty, 1878; Robinson, 1934) and our own observations, are:
1. Nomadic Afghan tribes (*powindahs*, or *kuchis* as they are often called in Bannu) which make annual migrations between Afghanistan and the Indian Sub-continent. They have done so for centuries, and they probably represent a lifestyle that has gone on for millennia. Their movements from Afghanistan take place in the autumn, to avoid the extreme cold and snows of winter.

> The happy possessors of cultivated lands and provision for the winter shut themselves in their houses and seldom venture forth; but the nomads of the black tents, for necessity of warmer climate and because they must continue to earn a livelihood throughout the year, have to move to lower altitudes. (Robinson, 1934: 1–2)

These nomads move with their families and flocks of animals, seeking pasture wherever it can be found, or purchased. Robinson (ibid. 2–3) identified various occupations of the *powindahs*: nomadic merchants who trade between Afghanistan and India and whose migrations are due as much to economic causes as to climatic pressures; nomadic pastoralists who are driven by climate to seek pastures on the other side of the frontier zone; and other categories of landless nomads who seek work in India (both of the latter categories also engage in small amounts of trade).
2. Settled Afghans who migrate to India (often leaving their families in Afghanistan) to trade or to gain winter employment.
3. 'A few men, such as the Ut Khel, who come to India in winter with the object of making a livelihood by theft' (ibid. 3).
All the groups of people in categories 1–3, above, migrate back to Afghanistan in the spring, to avoid the summer heat of the plains of the Indian Sub-continent, when the pasture lands become scorched (and when there would be competition with settled inhabitants for restricted pasture). These annual migrations have been severely curtailed in recent years because of the strife in Afghanistan; it will be interesting to see if this ancient lifestyle can ever be resurrected once peace is restored to the Afghan side of the Frontier.
4. Waziri semi-nomadic pastoral tribes which live in the valleys within the frontier zone, often extracting catch-crops (i.e. sowing and harvesting according to opportunity) from the alluvial soils of the valleys. These people regularly visit the plains for employment, trade and, until recent times (and even occasionally today), plunder.
5. Under present tribal conditions, it is rarely possible for settled inhabitants on the Bannu side to enter the frontier zone in order to engage in transhumant pastoralism. This pattern of exploitation, whereby a segment of a settled community takes their flocks into the hills for access to pastures during some of the hot dry summer months, should not, however, be ruled out for earlier periods (see below).
6. Commercial exploitation: the pine and cedar forests in the montane zones (above Wana, Parachinar, and in the Tirah) are cut for timbers which are transported down to timber merchants in the towns of the plains.

7. Criminals, especially murderers from towns and villages in the settled areas of Bannu, frequently become outlaws (*mufroor*) and flee to the hills of the frontier zone where they are relatively safe from the law, and from the vengeance of the relatives of their victim(s). This might seem to be a rather trivial category of 'exploiters of the frontier zone', but it is one which has a major impact on the ability of archaeologists to conduct surveys there!

We are not, of course, suggesting that all of these modes of exploitation were practised in later prehistoric times, but they do provide a useful range of models for interrogating the archaeological record.

Later prehistoric sites in the Bannu frontier zone

The known archaeological sites of all periods in Bannu Division have been reviewed by Khan (1986); subsequent discoveries of prehistoric sites, and of excavations at some of them, have been discussed by Khan et al. (1987, 1988, 1989, 1991a). To date, all the known later prehistoric sites in the Bannu basin are located in the north-west (Fig. 3). The various cultural phases represented by these sites, and their chronologies, are given in Table 1.

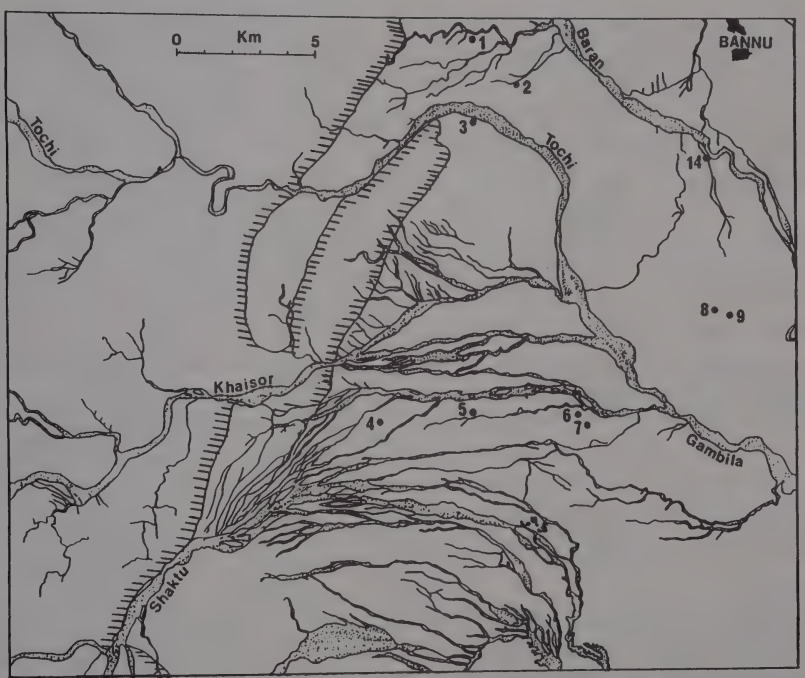

FIG. 3. Map of the northwestern part of the Bannu basin, showing the locations of the various later prehistoric sites mentioned in this paper (see Table 1 for details of chronology). The hatched line marks the eastern limit of the Hills of Waziristan. 2: Islam Chowki; 3: Tarakai Qila; 4: Tarakai Ghundai; 5: Sheri Khan Tarakai; 6: Barrai Khuarra 1; 7: Lak Largai; 8: Lewan; 9: Seer Dherai; 14: Ter Kala Dherai. (Site 1 is an early prehistoric site not discussed in this paper).

Although sites of Kot Dijian (sometimes termed Early Harappan) affinities are known in the Bannu area, there are no sites known of the mature (urban) Harappan phase.

The majority of the sites listed in Table 1 are small village sites which were probably occupied all year round. A few sites, notably Barrai Khuarra I and a number of small 'sites' to the west of Lak Largai (see Fig. 3), are only surface scatters of relatively small numbers of potsherds, and probably represent short-term occupations. These latter sites were located by intensive field walking. They have low visibility in the landscape (compared with the more settled sites, which are usually low mounds) and can only be discovered by walking over difficult terrain. This raises many logistical problems, the most important being security, in an area which has few 'jeepable' roads and which is heavily dissected by dry torrent channels (*nullahs*).

The Bannu Archaeological Project has been fortunate in having had the support of the administration of Bannu District (now Division) for its work. This has enabled us to extend our surveys and excavations away from the settled areas into Frontier Region, where we have made a number of significant discoveries, especially the site of Sheri Khan Tarakai

TABLE 1. Later Prehistoric Sites in the Bannu Basin.

Cultural phase	*Type site*	*Bannu sites*	*Date ranges**
Early Harappan	Kot Diji (Sind)	Islam Chowki	2500–2040 BC
		Tarakai Qila	None available
		Tarakai Ghundai	"
		Lewan	"
		Seer Dherai	"
Pre-Harappan (Chalcolithic)	Rehman Dheri (Phase l)	Islam Chowki	2870 BC
		Lak Largai	2885–2625 BC
		Lewan	None available
		Ter Kala Dherai	"
Early Chalcolithic/Late Neolithic	Sheri Khan Tarakai	Sheri Khan Tarakai	4240–2915 BC
		Barrai Khuarra I	None available
		Ter Kala Dherai	"

* Note: these calibrated date ranges are summaries of data derived from radiocarbon determinations of charred organic samples from various statigraphic units on each site. The radiocarbon determinations have been calibrated to calendar dates using the curve of Pearson et al. (1986). Details of the radiocarbon determinations on which the above summaries are based are given in Khan et al. (1991a), except for the new dates for Sheri Khan Tarakai (quoted here), which will be published in *Radiocarbon* in due course (Ambers et al., forthcoming).

(Fig. 3). This site, of the fourth millennium BC, has an assemblage of material culture which is unique and has been the major focus of our work since it was discovered in 1985 (Khan et al., 1986, 1990, 1991a, 1991b, 1992). It shows quite clearly the range of complex networks of social, and possibly economic, interactions which existed across a huge area of Central and northern South Asia in the fourth millennium BC. Although the inhabitants of Sheri Khan Tarakai appear to have been settled there for most, if not all, of each year, there is evidence of contacts with other local sites, as well as for the exploitation of a range of ecological zones (mostly in the frontier hill and montane regions), and for inter-regional contacts with populations in northern Baluchistan (to the south) and through to southern Turkmenistan to the north-west (Khan et al., 1992).

Exploitation of the Bannu frontier zone in later prehistory

Aspects of the agricultural exploitation of the area have been discussed elsewhere (Thomas, 1983a, 1983b, 1986a, 1986b, 1989; Khan et al., 1991c), and will not be considered further here—except for pastoralism. We focus on the evidence for the exploitation of different zones of the frontier, including the possible exploitation of the various passes for long-distance contacts, including trade.

Finds at most of the stratified occupation sites discussed above include fragments of both lapis lazuli and chank shell. These are sure indicators of some sort of exchange systems, if not of trade, with other populations outside the Bannu area. Chank shell has its origins on the coast of the Arabian Sea and shells (and sometimes finished shell artefacts, often bangles) have been traded or exchanged widely to sites far inland. The exact pathways of such movements of chank shell are almost impossible to discover.

The lapis lazuli found in the Bannu sites probably has its origins in north-eastern Afghanistan, but its route, or routes, of passage into Bannu cannot easily be defined. Lahiri (1992) has recently produced an interesting and ambitious review of the archaeological evidence for trade routes in India in ancient times. For prehistoric periods she attempts to source the raw materials used at various sites, and then to align trade routes between the source and the sites along the most probable (often the easiest or quickest) routes available. Even with detailed chemical and mineralogical data confirming the geological provenance of traded (or exchanged) materials (and Lahiri admits that such analytical data are rare for materials recovered from sites in South Asia), the exact alignment of routes for trade or exchange cannot be made. This is especially true when we are dealing with gift exchange networks reflecting social relationships, rather than with organized commercial trading relationships based on profitability and efficiency.

To return to the Bannu chank shell and lapis lazuli, it is probable that the routes of entry of these materials changed during later prehistory as a consequence of changing patterns of contact over this period. For the early site of Sheri Khan Tarakai, we have good evidence from distinctive 'alien elements' in the assemblage of pottery (Khan, Knox and Thomas, in preparation) for contacts with sites (such as Rana Ghundai) in northern Baluchistan. This could also be a line of contact along which chank shell gifts were exchanged into the Bannu area. In the Kot Dijian phase, however, potential contacts with other Kot Dijian sites to the

south might tempt us to align Kot Dijian exchange routes for chank shell from the coastal areas of Sind, through the Indus valley to the Gomal plain, and then through the pass at Pezu into Bannu. For lapis lazuli the situation is even more difficult. Present-day routes of entry of lapis lazuli into Pakistan from north-east Afghanistan are over the passes of the Hindu Kush into Chitral, and thence down to the Vale of Peshawar. This is a possible route for the material at Sheri Khan Tarakai, but we think that the Tochi route from Ghazni is more likely. Sheri Khan Tarakai is located close to the Tochi, and the terracotta figurines, and also the cones (Fig. 4), from the site indicate relationships with southern Turkmenistan, suggesting trans-Afghan connections to that part of Asia. However, such stylistic similarities in terracotta objects need not indicate trade or exchange, or any other social contacts with those areas, but cultural affinities rooted in common descent or kinship (Khan et al., 1992).

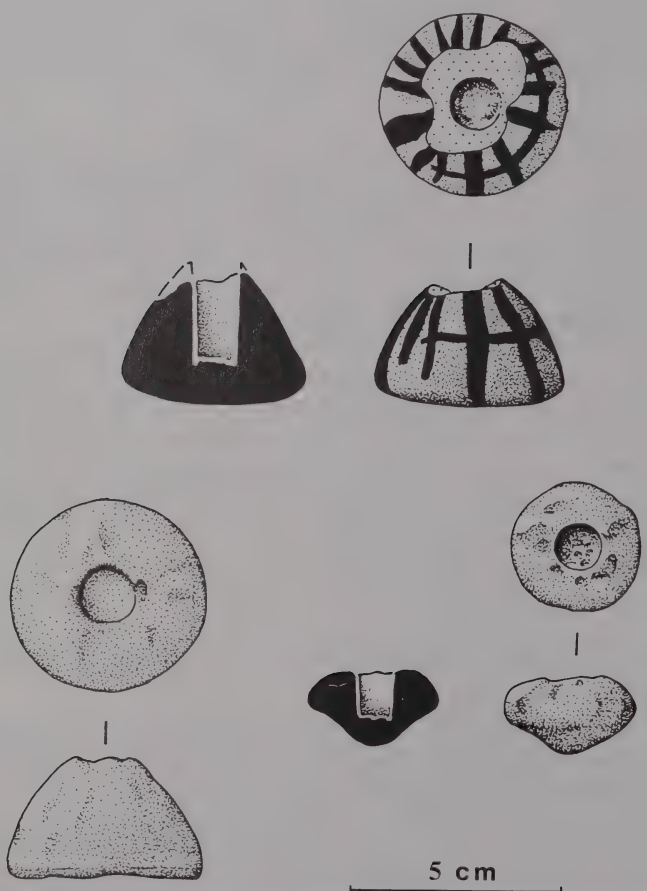

FIG. 4. A selection of partially perforated terracotta cones from Sheri Khan Tarakai, Bannu.

Other patterns of exploitation of the frontier hill and mountain zones might also be inferred from the data recovered from excavations of sites in Bannu. The representations of wild mountain goats on pots made at Sheri Khan Tarakai (Fig. 5) suggest affinities with the uplands of the frontier zone; representations of animals on the later Kot Dijian phase ceramics, on the other hand, are almost entirely limited to domesticated cattle or buffaloes, or to the fish of the rivers of the plains. Exactly what this might mean in terms of the perceptions of the potters of Sheri Khan Tarakai is not entirely clear. Perhaps they believed their origins to have been in the hills and mountains, and this influenced the designs on their pots. Perhaps groups of people from Sheri Khan Tarakai were regularly visiting the uplands, for pastoral purposes with their flocks, or to hunt wild goats. The mobility of people with cultural affinities with Sheri Khan Tarakai is shown by the temporary occupation site at Barrai Khuarra I, located on a terrace of the same torrent on which Sheri Khan Tarakai itself stands (Fig. 3). Incidentally, this torrent issues from the hills through the Seine Tangai, a small pass which, in addition to giving access to some inner valleys with good summer grazing potential, also has a series of small springs which remain active through much of the year.

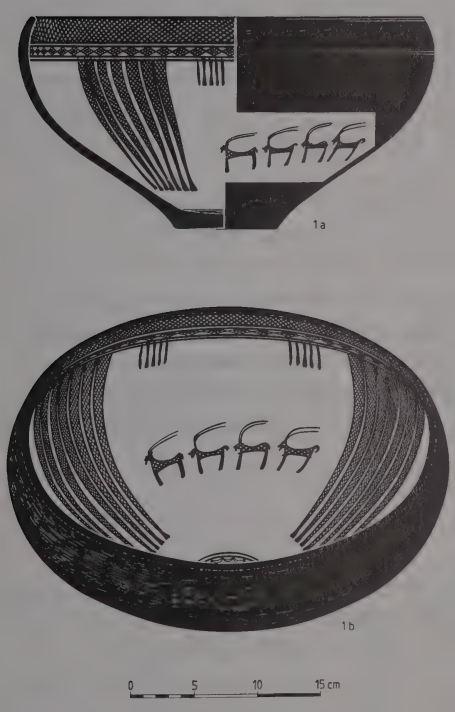

FIG. 5. Profile (1a) and general view (1b) of reconstructed wide-mouthed bowl from Sheri Khan Tarakai, Bannu. The exterior surface of the bowl is painted black. The internal surface is covered with a burnished pinkish-buff slip and painted with black designs, including mountain goats.

Another source of evidence for the exploitation of various of the frontier zones by the inhabitants of Sheri Khan Tarakai comes from identifications of the wood charcoals recovered from the site. These studies are at an early stage, yet they give clear indications of wood resources being brought to the site not only from the local vicinity but also from the low hill, the higher hill and the lower mountain zones (Cartwright and Thomas, in preparation). Woody taxa identified so far include *Tamarix*, *Calligonum*, *Salvadora* and *Zizyphus* (representative of the dry thorn habitats around the site today), *Acacia*, *Olea*, *Celtis*, and *Pistacia* (taxa from the low and higher hill zones), and *Quercus* (from the temperate dry oak forest, up to altitudes of 2,500 m). Wood from most of the local taxa must have come to the site as fuel or for building materials, but that from taxa which occur well into the frontier zone could have been brought in (or acquired by exchange) as

artefacts. More work needs to be done on the wood charcoals from Sheri Khan Tarakai, and on the samples of charcoals from other prehistoric sites in the Bannu area, to investigate the landscape units being exploited in the past.

CONCLUSIONS

A major problem in trying to detect past use of the Bannu frontier zone, involving either local exploitation of resource zones or exploitation of passes for longer distance trade, exchange or migration, is that we are able to work only on one side of the frontier. We have little evidence of settlements of the later prehistoric periods on the Afghan side of the frontier, and we are unlikely to be able to undertake site surveys within even the Pakistan side of the frontier zone itself. Remote sensing, e.g. by satellite imagery, is unlikely to be of help: most of the sites in the frontier zone are likely to be small and temporary occupations, with low visibility, and dating any sites spotted would be impossible unless arrangements could be made for artefacts to be collected from them. We are therefore forced to continue with our approach of examining the spatial patterning and particular locations of different functional categories of sites on the Bannu side of the frontier zone, especially in relation to possible routes of movement, and of using artefact styles, raw materials and environmental data to detect contacts, at various spatial scales, with zones within the frontier and areas beyond it.

Despite the problems outlined above, we believe that there is growing evidence to show that inhabitants of the Bannu basin were deriving various benefits from their proximity to the hilly and mountainous frontier zone by at least the fourth millennium BC. These benefits included access to upland summer pastures, a diversity of wood resources, and items brought across the frontier zone by migratory groups (perhaps following lifestyles similar to the recent *powindahs* discussed above). The presence of settled farming communities on the Bannu side of the frontier could have been a spur for (possibly related) nomadic herders from what is now the Afghan side to take advantage of winter pastures there, and to bring items for trade or exchange.

Abstract

The passes on the North-West Frontier of Pakistan have been important for millennia as routes of invasion, migration and trade. This paper considers the frontier zone adjacent to Bannu Division, North-West Frontier Province, in relation to: its physiographic characteristics and natural resources; how it was exploited in the historic past; and what evidence there is for its exploitation in the later prehistoric periods. It is suggested that exchange networks extending in various directions beyond the frontier zone were well established by at least the fourth millennium BC, and that resources within the frontier zone were also being exploited by nomadic and/or transhumant pastoral groups.

Acknowledgements

We are deeply grateful to our friend and collaborator Professor Farid Khan for his support and encouragement over the years. We thank Professors Abdur Rehman and Farzand Durrani of the University of Peshawar, Dr Jehan Dar Shah of Islamia College, Peshawar, and Mr Feroze Khan of Bannu City for help in many ways. Dr Rafique Mughal and Professor A. H. Dani have been sources of valuable suggestions. Janet Ambers and Caroline Cartwright, of the Department of Scientific Research at the British Museum, undertook the radiocarbon determinations and the analysis of the wood charcoals, respectively. Mr Graham Reed helped with the figures. We gratefully acknowledge the main financial sponsors of the project: the Trustees of the British Museum and the Society for South Asian Studies (British Academy).

Endnotes

1 In previous publications by members of the Bannu Archaeological Project, directed by Farid Khan, J.R. Knox and K.D. Thomas, we have referred to Bannu as a District; this has now been elevated to Divisional status.

2 Although we refer in this paper to Bannu as a destination, we really mean the Bannu basin, especially that part to the west of the town of Bannu. The modern town of Bannu dates from AD 1848; it was founded by Herbert Edwardes and was known for some time as 'Edwardesabad'.

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Some problems of the Middle and Late Bronze Age in southern Albania

by LORENC BEJKO*

Southern Albania has a varied natural environment with both mountainous and lowland coasts, and a rugged interior divided by wide valleys leading to Epirus and western Macedonia as well as fertile basins. Placed on the Adriatic coast, with a key position on the sea-routes linking the Balkans to the Italian heel and the northern Adriatic to the Aegean world, and having good natural routes leading to other parts of the Balkans, it can be expected that this area has experienced many different cultural influences.

Some natural minerals and ores can be found in this part of Albania, including asphalt from Selenicë in the Shushicë valley, copper and iron in the northern part of the Korçë Basin as well as copper from the Mati valley region and from Gjegjan in north-east Albania (the last two sources are some miles north of the region included in this study). The presence of these resources have been important for trade and exchange with developed centres in southern Greece and the Aegean, as well as with southern Italy and the Balkans.

THE PRESENT STATE OF RESEARCH

Much thorough research and excavation has been undertaken in this region. Professor Prendi excavated the very important site of Maliq, which has provided a stratigraphical sequence for the Bronze Age (1966), as well as the interesting tumuli at Vajzë (1957), Vodhinë, Bodrishtë and Kakavijë (Prendi and Budina, 1970). Professor Islami and H. Ceka excavated and published materials from the Pazhok tumulus (1964), Dr Andrea, the tumuli at Barç and Kuç i Zi (1985), Dr Korkuti the settlements at Tren (1971), Bënjë and Katundas and the tumulus at Patos (1981). Dr Budina excavated the tumulus at Bajkaj (1971), Dr Bodinaku the tumuli at Piskovë, Rapckë (1981) and Këlcyrë (1988), Dr N. Ceka the tumulus at Dukat (1974), Dr Aliu the tumuli at Rehovë, Prodan (1984) and Luaras, and Dr Lera the settlement at Sovjan (1990). Professor Hammond surveyed much of the area and reported some finds in *Epirus*, and later was involved in the discussion of some groups of materials from the tumuli at Vajzë, Vodhinë and Pazhok (1967a: 310-40). There are many stray finds from this region; the number of archaeological finds is remarkable and compares well with northern and north-west Greece. A general overview on the Bronze Age in this part of Albania has been carried out by Prendi (1982a: 214-28). Some of the objects discovered have been included in

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discussions of particular classes of finds, such as weapons (Catling, 1968), tools (Harding, 1984: 240) or jewellery etc.

Settlements

Unfortunately, there is insufficient evidence from the settlements to establish any pattern. Figure 1 gives an impression, however, of the frequency of settlements of the Middle Bronze Age (MBA) and Late Bronze Age (LBA) in southern Albania that suggests an obvious growth of population compared to the Neolithic and the Early Bronze Age (EBA). Coastal sites and sites around the Korçë Basin are relatively frequent as well as those in wide and fertile valleys. Acropolis hilltop sites, attributed to the LBA, occur around the Korçë Basin (Trajan, Tren, Ventrok, Luaras etc.) and on the coast (Badhër, Karos, Borsh, Pirg, Triport etc.) and Mycenaean finds can usually be found.

The Bronze Age site at Triport is situated at the summit of the hill that marks the begining of the range from north of the Gulf of Vlorë. There were no traces from the Bronze Age settlement, but judging from the distribution of the finds, the excavator Bereti concluded that the site was originally restricted to a small area, but expanded down the slope of the hill during the Archaic period (as much Corinthian pottery has been reported) and the Classical period (Bereti, 1985). Professor Hammond had visited the site before World War II and retrieved some Mycenaean sherds which he reported later in *Epirus* (1967a: 313), but no illustrations or further descriptions were given. More careful exploration of the site is needed before one can judge the nature and importance of this settlement. It is very probable, however, that the Gulf of Vlorë was known by traders, at least from the Late Neolithic onwards, suggested by the material from the Velçë cave (ibid. 294) and that Triport served as a station point on the way to the northern Adriatic or southern Italy. The asphalt from Selenicë was sought after and the trade in that may have been related to this coastal site.

Some interesting evidence came from the excavations of the settlements of Badhër and Karos. Korkuti has briefly described both sites, which are excellently illustrated with plans and photos (1982), reproduced here (Figs 2.1 and 2.2). No Mycenaean finds were found at either site; all the coarse and fine pottery reported was local. It is likely that these sites belonged to small pastoral communities and that the area enclosed by walls (especially in the case of Badhër), was reserved for sheep rather than for defence.

The settlement at Tren is unfortunately less well known. Some Mycenaean pottery was found. It has been possible to discover some parts of the surrounding wall, but neither a plan nor any details of the internal organization of the site are yet available.

Undoubtedly, a great need for defence must have been the main reason for the appearance of this kind of walled settlement in the LBA. This idea is supported by the relatively great number of weapons reported from LBA sites and graves.

One of the most important Bronze Age sites in southern Albania, that of Maliq, was an unfortified settlement situated in the Korçë Basin, surrounded by fertile arable land and located fairly close to water. These two factors appear to have been more important criteria for location than defence in this case. Caves continued to be used, as at Tren, Katundas, or Konispol etc.

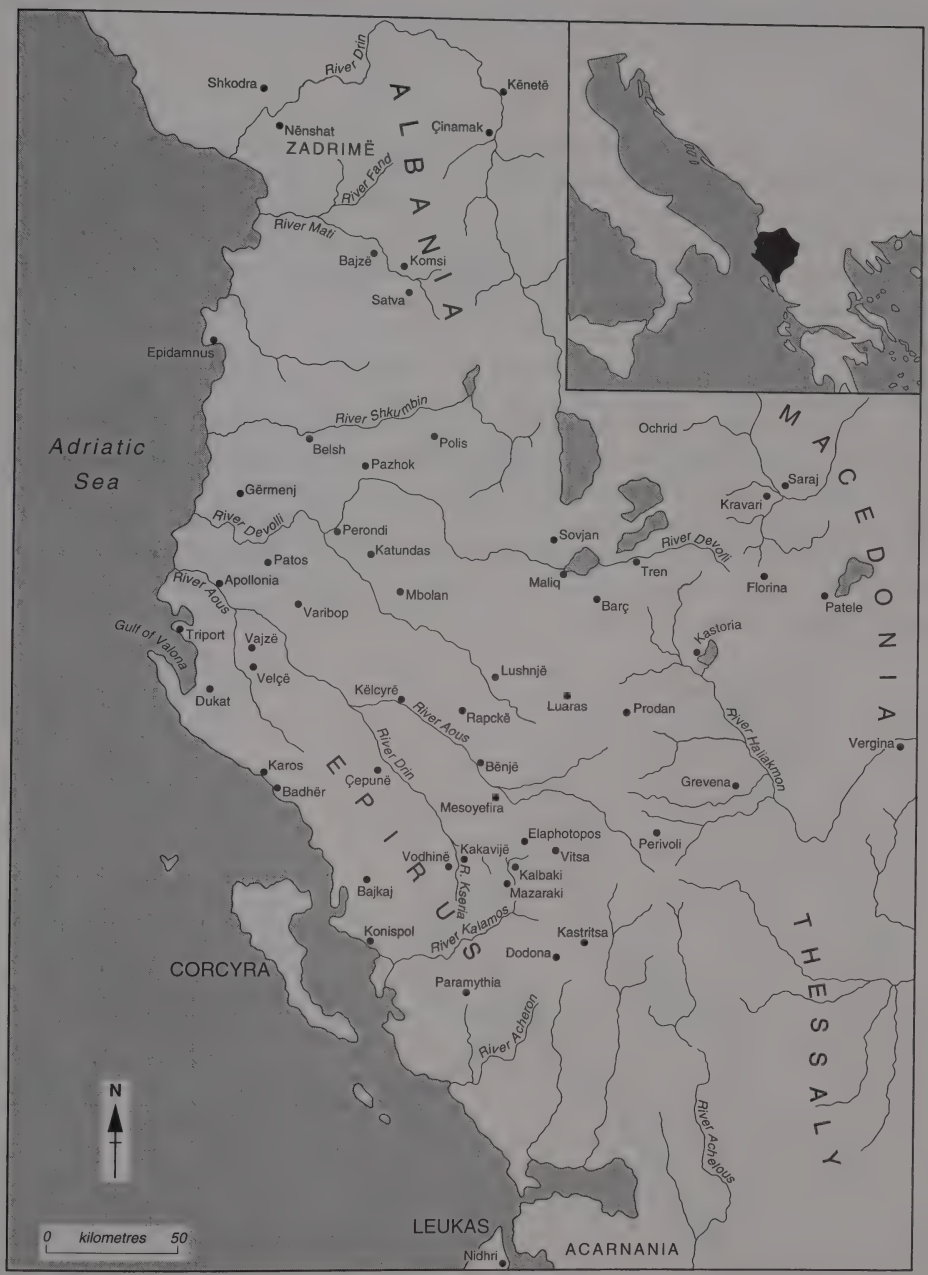

FIG. 1. Middle and Late Bronze Age settlements in Albania, Epirus and western Macedonia.

It is not always possible to relate tombs or stray finds to the settlements, although thay can suggest the existence of a nearby settlement or the inhabitation of the area.

Houses

Little is known about the appearance of Bronze Age houses or how they were constructed. At Maliq, material from the Bronze Age levels shows that the houses were built in the Neolithic tradition, consisting of a largely rectangular shape, with one or more rooms constructed of woven branches or reeds coated with mud. They were equipped with one or more hearths. Although this traditional pattern predominates, a rectangular hearth with kerbs on one side for storing logs, and a hollow space below to provide a draught has been found at this site for the first time (Prendi, 1982a: 232).

Other house types are known at Badhër and Karos. At Badhër, 16 house remains were discovered that were not placed within the encircling walls (in fact, no house remains were discovered within this area). The construction of the houses incorporated natural outcrops of rock. According to their layout, they fall into two groups: those of rectangular plan, as in the case of House 15 (Fig. 3.1), and those of circular plan, as in case of House 5 (Fig. 3.2). House 15 is 10.2 m long and 6.2 m wide. As can be seen in the illustration, it consists of two rooms. The western wall is well preserved, from 0.5 to 0.6 m high and from 0.8 to 1.0 m wide. House 5 has a diameter of from 5.6 to 7.0 m, containing a single room, and the wall is preserved up to a maximum height of 1.2 m and a width of 0.7 to 0.8 m (Korkuti,1982: 244–5).

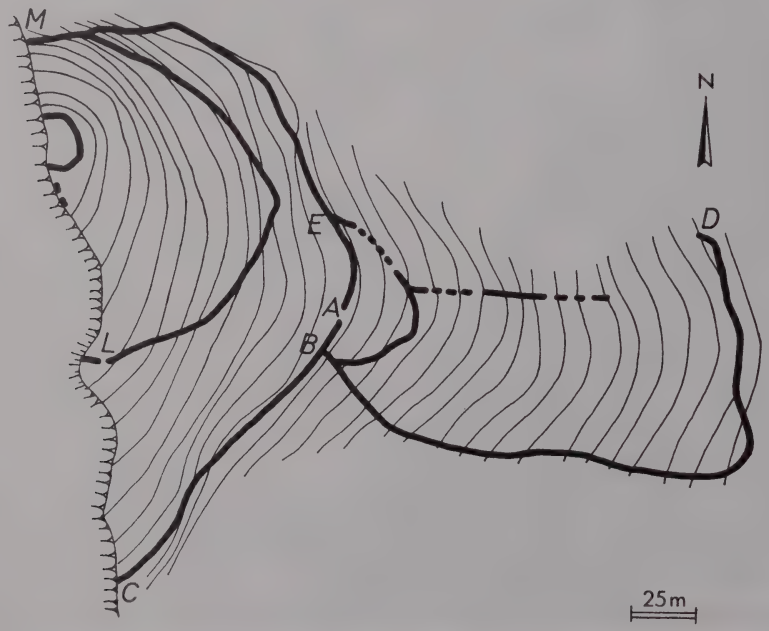

FIG. 2.1. Settlement plan of Karos.

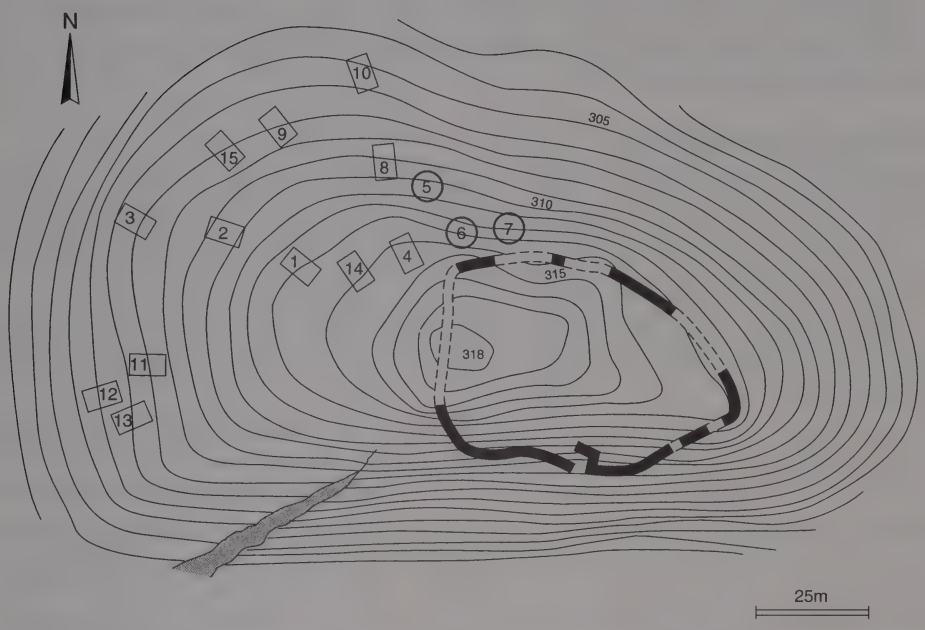

FIG. 2.2. Settlement plan of Badhër (after Korkuti, 1982, abb. 2 and 4).

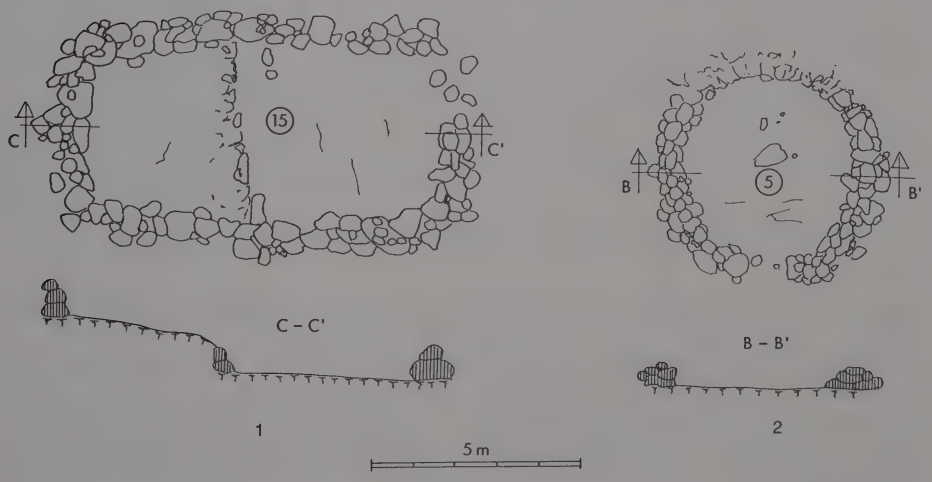

FIG. 3.1. Rectangular plan of House 15 at Badhër.
3.2. Circular plan of House 5 at Badhër (after Korkuti, 1982, abb. 8).

A miniature terracotta model that was found in the earliest Bronze Age levels at Maliq suggests what the oven may have looked like. It had a horseshoe shape, consisting of a hearth in front of the opening fitted with two cylindrical chimneys to direct the smoke to above the roof (Prendi,1982a: 232).

Tombs and burial customs

Tombs are better represented and better known than the settlements. As we can see from Figure 1, they have been discovered in great numbers. Even Bronze Age material culture is better known from mortuary rather than settlement artefacts. Large cemeteries have usually been discovered, but there are also some isolated graves and graves in small groups.

As in earlier periods, the tradition of flat cemeteries such as Gërmenj, Drenovë and possibly Katundas, continued to be used. Whether this is a characteristic of the western part of the area under discussion or whether this reflects a lack of evidence from other areas, is too early to say.

Tumuli have proved to be more frequent however: e.g. Pazhok, Patos, Vajzë, Dukat, Bajkaj, Çepunë, Vodhinë, Bodrishtë, Kakavijë, Piskovë, Rapckë, Prodan, Luaras, Rehovë, Barç, Kuç i Zi, or Cerrujë. It is not my aim in this paper to describe in full the architectural elements of these tumuli; there are good published descriptions for each of them, as well as some general studies by Hammond (1967b) and Prendi (1977-8). They were usually constructed over a central grave with other surrounding graves. At Pazhok, Vajzë and Vodhinë, the earliest graves can be dated without doubt to the MBA; and later ones to the LBA or even the Early Iron Age (EIA). It is evident that the most intensive use of the tumuli began in the LBA and EIA.

The excavator of the tumuli at Barç, Dr Andrea, suggested that the date of the central grave in Tumulus I fell within the EBA (1985: 99). This date, based on the similarities of the pottery from this grave to the pottery of Maliq phase IIIa has not greatly convinced me. Nevertheless it is supported by other Albanian archaeologists and is considered as evidence for an early date for the appearance of tumuli in southern Albania (Prendi, 1982a: 235).

The Albanian tumuli are of great importance as they have yielded a great deal of information about the material culture of the MBA and LBA, and the burial customs of these periods. A northern origin for this burial custom, which can also be found on the western and southern Greek mainland, is probable. The three tumuli recently found at Ephyra and two others at Pogoni, make it clear that this burial practice was known at Greek Epirus from at least the LBA.

Apart from large cemeteries dated to the Bronze Age, there are also some isolated graves or graves in small groups (as at Pëllumbas or Varibop, for example).

Excavations of flat cemeteries, tumuli, and stray finds, have shown that there were various grave types: simple pit-graves (as at Barç, Pazhok or Gërmenj); graves lined with small stones (at Barç, Dukat, Pazhok); cist-graves (at Vajzë, Bajkaj or Katundas); graves of wooden construction (as at Pazhok); and urns. Normally, graves were of rectangular or circular form.

It is very difficult to relate the different kinds of graves to different geographical areas

within southern Albania, or to any particular period within the MBA and LBA. It can be stated however that simple pit-graves are known to be earlier than cists, but both continued to be used together later in the LBA and EIA. From the viewpoint of material culture, it seems likely that the various types of graves were used contemporaneously by people from similar, if not the same, cultural groups.

The general burial custom was inhumation; this was the traditional mode of burial and was very common in southern Albania during both periods. The orientation of the body does not appear to have been fixed. Occasionally, two or more skeletons have been found in the same grave. Cremation was more unusual, but is known in MBA tumuli at Vajzë and Vodhinë, and in the LBA graves of Tumulus I at Barç.

Graves usually contained some goods (pottery, weapons, tools, jewellery, etc.), but there are also cases of graves without any associated artefacts. Animal bones discovered inside or outside some graves have been interpreted as remains of animal sacrifices, and sherds found over some graves as being due to a ritual of pottery breaking, which may have been part of the burial practice (Islami and Ceka, 1964: 96).

THE MIDDLE BRONZE AGE

I will consider two classes of material from the MBA: pottery with grey fabric, and bronzes including long swords, daggers, knives and spearheads.

Grey fabric pottery

This type of pottery, from southern Albania, is handmade, and is usually known as 'pseudo-Minyan' pottery, i.e. Minyan pottery of non-classical production.

The unenclosed settlement at Maliq (already mentioned above as one of the most important sites to provide a stratigraphical sequence), yielded some of this pottery type. The excavator of this site, Prendi, describes the pottery of the MBA stratum (Maliq IIIc) as generally made with greater care than before, mostly grey, dark grey or black, with a smooth surface, sometimes even burnished (1982a: 214). A full publication of the objects from this important site is needed before it can be suggested that the characteristics of this MBA pottery reflect influences from Grey Minyan pottery of Thessaly or central Macedonia, even though this seems likely.

One of the better represented shapes from Maliq is the *kantharos*, which may belong to the Shaft Grave period in mainland Greece (Fig. 4.1). Nevertheless, other earlier shapes were also in use, according to the published drawings.

Another *kantharos* is known from the tumulus at Vajzë, which is associated with a long sword of Sandar's type A (Fig. 4.2). The association of this vessel in this grave helps to pinpoint a more exact date that may be 1600–1550 BC. I think that here we have another example of the late phase of Minyan pottery. Taking into consideration the established and continuous contacts between western Greece and the Ionian Islands with south-west Albania (there are indications of this from at least the Late Neolithic as the painted pottery from the Velçë cave suggests), the date of late phase Minyan pottery in Albania, if not contemporary, differs little from that of western Greece.

There are other examples of this class of pottery from the Drinos Valley tumuli. The most important evidence comes from the tumulus at Vodhinë. In Cremation 16 of this tumulus an early Minyan pottery form was associated with a Middle Helladic bronze dagger (Prendi and Budina, 1970: 63). The spherical bowl with everted rim, flat base and two small vertical handles below the rim is a well-represented shape from this tumulus (Figs 4.3, 4.4 and 4.5) and in one case there was incised decoration below the rim (Fig. 4.6), which may have been a non-classical Decorated Minyan representation. Other late Middle Helladic pottery shapes known from this site are the dippers (Figs 4.7 and 4.8). I believe the details of shape and decoration tie in satisfactorily with well-dated material from grave groups and the long-term contacts between the area under consideration and western Greece.

Other examples of Grey fabric pottery can be seen in Budina's publication of the Bajkaj tumulus in the Delvinë plain (1971), such as one-handled small spherical cups (Fig. 4.9), bowls with two small vertical handles below the rim (Figs 4.10, 4.11), dippers (Fig. 4.12) and *kantharoi* (Fig. 4.13). The Delvinë plain is quite close to the coast, so this tumulus has provided good evidence for the maritime links of western Albania to the Ionian islands, seen more clearly in the LBA. Other finds in the Apollonia area (Korkuti, 1970, pl. IV), and at the cave at Katundas have added to the known distribution of this type of pottery in southern Albania.

As we have already seen, non-classical Minyan pottery production is well represented in the western part of southern Albania, which suggests a maritime influence, and in the Korçë basin; this influence appears to have been from MBA Thessaly. The discoveries in the Drinos Valley are more problematic in terms of pin-pointing the direction of influence; it is more likely that this was from south-west coastal Albania rather than from Epirus.

In the *Prehistory in Albania,* Professor Prendi stated that 'a *terminus ante quem* for the first appearance of a "pseudo-Minyan" type in Albania is furnished by a specimen from Vajzë (Vlora), found in a tomb in Tumulus I, together with a sword of Aegean type of the period 1700–1500 BC. In view of this association, this type of vessel in Albania originated at an earlier date, but not earlier than the eighteenth century BC' (1982a: 216).

I agree with Prendi in taking Grave 16 in Tumulus I at Vajzë to give a *terminus ante quem* for the appearance of non-classical Grey Minyan pottery in southern Albania, but dating this type of pottery in Albania to not earlier than the eighteenth century BC does not seem well founded. As I have tried to argue on a typological basis, it seems to me that this date could well be even earlier, in the nineteenth century BC.

Middle Bronze Age bronzes

A number of bronzes have been discovered from MBA graves in southern Albania, such as long swords, daggers, knives and spearheads. These objects have long since been defined by Prendi and Korkuti as being related to the Aegean cultural sphere. They are all well described and illustrated in various publications (Hammond, 1967a: 328–31; Korkuti, 1970; Prendi, 1982b: 213–14), and several scholars have used some of them in their more general studies on specified groups of bronzes (Branigan, 1974; Kilian, 1985). I shall provide short descriptions of these objects, and a re-consideration of some of them.

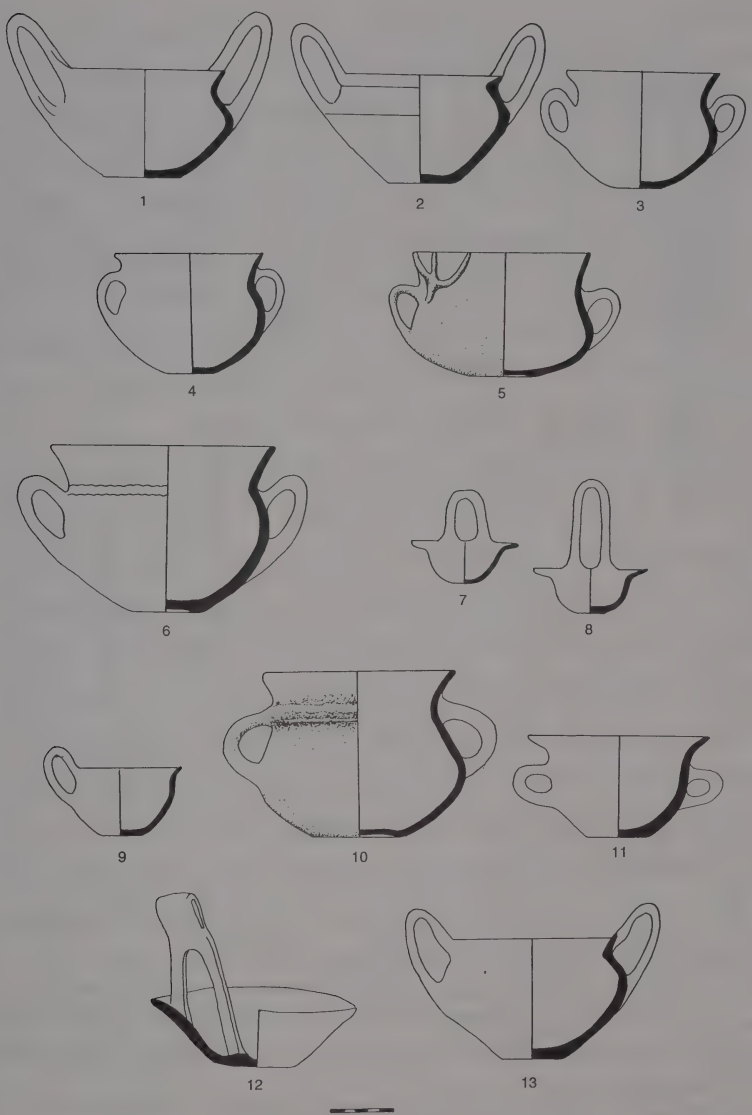

FIG. 4. Grey fabric pottery, pseudo-Minyan (after Prendi, 1982b, fig. 5).
4.1 *Kantharos*, Maliq.
4.2 *Kantharos*, Vajzë.
4.3–5 Spherical bowl, Vodhinë.
4.6 Decorated spherical bowl, Vodhinë.
4.7–8 Dippers, Vodhinë.
4.9 One-handled small spherical cup, Bajkaj.
4.10–11 Two-handled bowl, Bajkaj.
4.12 Dipper, Bajkaj.
4.13 *Kantharos*, Bajkaj.

Long swords

A long sword from Vajzë, Tumulus I, Grave 12 (Fig. 5.1) was found in six pieces, measuring 1.01 m in length, with a high midrib profile. The midrib has incised parallel lines, is straight with almost parallel edges, has narrow, squared shoulders and a short oblong tang, with two rivets on the tang and one in each shoulder (the rivets are 0.017 m long). This sword clearly displays features of Karo-Sandars Type A. It is classified in various publications: Hammond related it to a similar type but shorter variety found at Leukas Grave R 7; in the shaft graves at Mycenae (1967a: 320), Branigan included it in his Type III long swords together with the two Mallia swords found in the debris of the Middle Minoan (MM) IIb phase (1974: 17). It seems probable that this sword can be dated to the Middle Helladic (MH) III, which is the date of the appearance of this type of sword for the first time on mainland Greece. It is true that there is evidence for the use of the Type A sword as late as the Late Helladic (LH) II or even LHIIIA–1, but the association of this sword in a grave with a MH knife and a Minyan *kantharos* restricts its date to within the seventeenth century BC.

The sword from Pazhok, Tumulus I, Grave 7 (Fig. 5.2) was found with a *vapheio* cup of LHIIA date, and has been variously classified. Professor Hammond considered the strong medial rib on the blade, the slight expansion near the top and the position of the rivets to indicate that the sword was an early specimen of the cruciform type that belongs typologically to LHI or LHII (1967: 82). Professor Prendi considered it to be a horn-handled sword, which at first sight seems to be of the same type as the Aegean swords of Sandar's Type C, but in fact there are marked differences in the shape of the blade and in the horned wooden handle carved separately and attached to the blade by many rivets. Considering it to be of local production, Professor Prendi saw the Pazhok sword as being without parallel in the Aegean world and the surrounding area giving it a special place in the evolution of Bronze Age swords in Albania, from the Vajzë sword to the classical horned sword (1982a: 218). As we can see in both cases this sword has been classified on the basis of the hilt shape, neglecting the blade. Professor Prendi was correct when he considered the blade of this sword to be different from other swords of Sandar's Type C, but he saw this as a local feature, which seems unlikely to me. Taking into consideration the association of this sword with the *vapheio* cup of LHIIA type, both scholars speculated that it was an early specimen of the horned or cruciform swords respectively. I would agree with Imma Kilian who considered at least the blade of this sword to be Aegean, and judging from the incised lines of the midrib, even to be Cretan (1985: 252–3). The handle may be a local production of the LHI period or even later, and it must be stressed that the appearance of the handle of a Type A sword is not yet known.

The Midhë sword, from Tumulus I (Fig. 5.3) is generally considered to be the same as the Pazhok sword (Prendi, 1982a: 218). I think that this is right. Both swords are very much the same, with some unimportant differences, including a smaller number of rivets and a length 50 mm shorter on the Midhë sword. They both suggest the existence of local bronzesmiths but it seems unlikely that the technology being used was developed enough to produce such blades, so I would consider them to be Aegean imports.

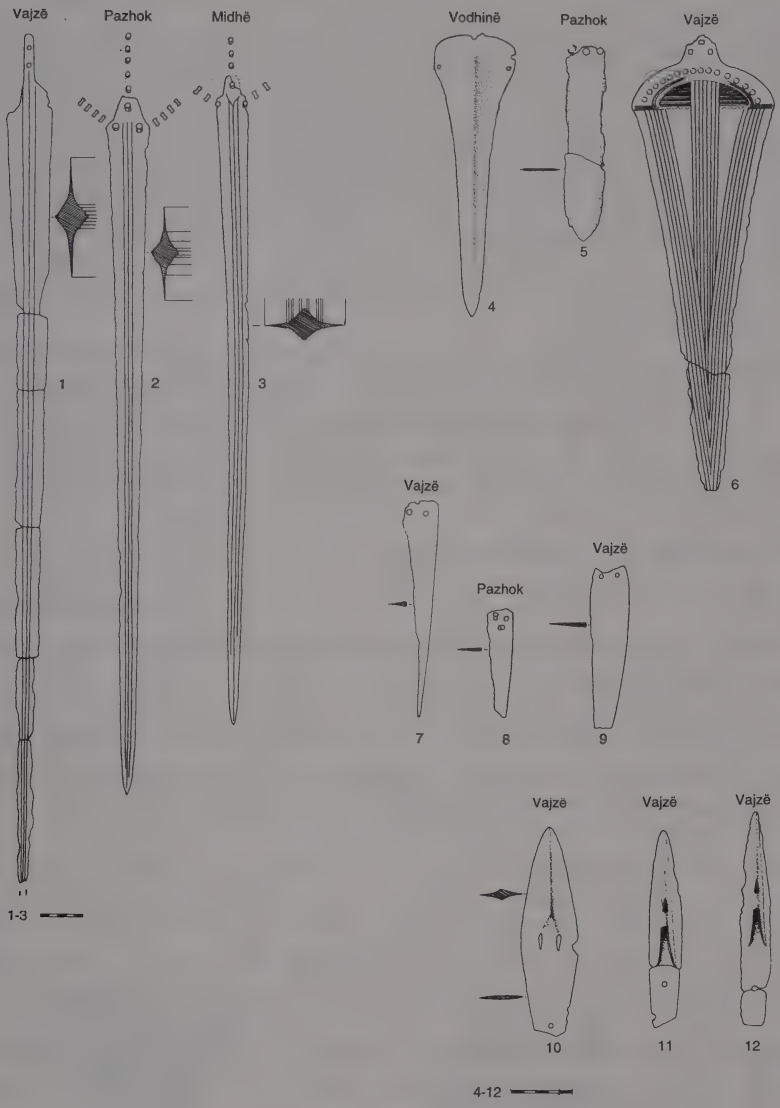

FIG. 5. Middle Bronze Age bronzes (after Prendi, 1982b, fig. 6).

5.1 Long sword, Grave 12, Tumulus I, Vajzë.
5.2 Long sword, Grave 7, Tumulus I, Pazhok.
5.3 Long sword, Tumulus I, Midhë.
5.4 Dagger, Grave 16, Vodhinë.
5.5 Dagger, Tumulus I, Pazhok.
5.6 Dagger, Grave 14, Tumulus I, Vajzë.
5.7 Knife, Grave 12, Tumulus I, Vajzë.
5.8 Knife, Tumulus I, Pazhok.
5.9 Knife in soil, Tumulus I, Vajzë.
5.10 Slotted spearhead, Grave 12, Tumulus I, Vajzë.
5.11 Shoe-socketed spear, Grave 12, Tumulus I, Vajzë.
5.12 Shoe-socketed spear, in soil of Tumulus I, Vajzë.

Daggers

There are four daggers recorded from southern Albania. One of them comes from the Vodhinë tumulus, Grave 16 (Fig. 5.4). It is 0.24 m long, with a rather accentuated middle spine, round shoulders, a ridged profile, concave edges and two rivets. It shows all the characteristics of the Minoan types, dated to the Early Minoan and Middle Minoan (EM II, MM II). This was included by Branigan in his Type IIIb long daggers because of analogies to the dagger found at Lerna (1974: 9, no. 176). None of the long daggers included in this type are unequivocally associated with pure EBA deposits. From their basic shape, however, and simple riveting system, he dated at least some Cretan and Cycladic examples to within the EBA. The Vodhinë dagger, associated with Grey Minyan pottery, must be dated to the Early Middle Helladic (EMH).

The Pazhok dagger from Tumulus I (Fig. 5.5) is 0.17 m long, of ogival shape with a straight top fitted with three rivets. Prendi gave a LHI date to this dagger because its cutting edge is treated using the same technique as that of a knife found also at Pazhok, in a LHI context (1982a: 218). Nevertheless, the excavators (Islami and Ceka, 1964) dated it, and all the associated material, to within the MHII period of 1800–1700 BC, which seems more probable.

The dagger from Vajzë, Grave 14, Tumulus I (Fig. 5.6) is 0.38 m long, of triangular shape, with a kite-shaped blade, and is decorated with three sets of incised parallel lines. Two sets run down the cutting edges and one set down the upper medial part, where it meets the other two sets about halfway down the blade; there are eight parallel lines in the central set. There are three fine rivets in a diminutive tang. A hollow-based hilt was fastened to the blade by very small rivets. Hammond related its shape and decoration to daggers from Parma of the Italian EBA (1974: 141), which I concur with. This example clearly has elements in common with European and especially Italian EBA daggers, and together with the bronze belt ornament decorated with similar engraved lines from the tumulus at Çinamak (north-east Albania), indicates contacts between the two sides of the Adriatic.

Another bronze dagger was found in the soil of Tumulus A at Vajzë (Prendi, 1982a, fig 44.6). It is 0.15 m long, of rhomboid section with three relatively large rivets in a triangular position. Keith Branigan included this dagger in his Type XIIIa together with similar daggers from Amorgos, Poliochini, V829 and Lerna. This group is characterized by a thickened profile, concave edges, a rounded base and a large heel projection in which the third rivet is situated (Branigan, 1974: 12). The Vajzë dagger has a closer parallel in the Lerna dagger of the same type (1974: pl. 8.306) which is securely dated to a MHI context. Hammond gave another parallel to a bronze dagger 0.12 m long, found in Grave F7 at Leukas of MMI–II date (1967a: 330).

Knives

Of MBA date are some bronze knives reported from the tumuli at Vajzë, Pazhok and Mati (Fig. 5. 7–10). These mark the first appearance of this kind of tool (which may also have been used as a weapon), in southern Albania and which continued to be used, with several variations, throughout the LBA.

The knife that came from Tumulus I, Grave 12 at Vajzë (Fig. 5.7), is 0.175 m long, straight-backed, and has three rivet holes. It falls to Branigan's Type V together with specimens from Amorgos, Dodona and Stavros (Ithaca). Of all the examples of this type, only the Vajzë knife is dated by its context to within the MH period.

The knife from the Pazhok Tumulus I also appears to be of this type and date (Fig. 5.8). Two other knives from the soil of the Tumulus I at Vajzë are of Branigan's Type Va. The one shown in Figure 5.9 is 0.135 m long, but the other only survived in fragments. They both have a slightly convex back, but in contrast to the Type V examples have only two rivets across the blade.

Spearheads

Three MBA spearheads are known from Tumulus I at Vajzë. One slotted spearhead was found in Grave 12 (Fig. 5.10) together with a shoe-socketed example (Fig. 5.11); another shoe-socketed spearhead comes from the soil of this tumulus (Fig. 5.12).

The slotted spearhead of Grave 12 belongs to Branigan's Type IIa with parallels from Mallia, Crete and Amorgos for example, generally dated to MMI–II. The shoe-socketed spearheads belong to Branigan's Type Va because there is only one hole between shoe and heel, and have very good parallels from Sesklo, Leukas (Grave F7) of basically MH date.

Mycenaean Pottery

The earliest evidence of Mycenaean pottery imports to southern Albania comes from Tumulus I at Pazhok (Fig. 6.1). Here a *vapheio* cup was discovered in the same grave as a sword of Aegean type (discussed above). This is a Type III *vapheio* cup shape with a small clean-cut base, high cylindrical lower body and very flaring upper body, with a strap handle that ends halfway down the upper body. The central rib is shallow, and is decorated in both zones, above and down the rib, with ripples. The interior rim band is neatly finished. These details

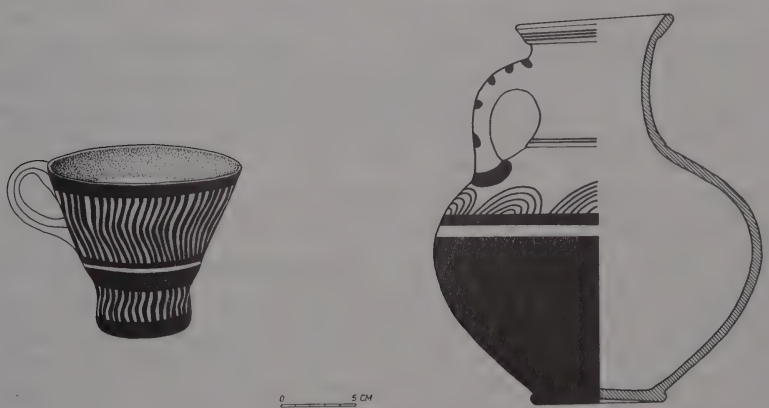

FIG. 6. Mycenaean pottery. 6.1 *Vapheio* cup, Tumulus I, Pazhok. 6.2 Painted *lekythos*, Grave 14, Tumulus I, Bajkaj. (Drawn by Ilir Zaloshnja.)

of shape and decoration suggest that this *vapheio* cup is very probably of LHIIA date (for more details of shapes and motifs of *vapheio* cups see Mountjoy, 1986: 33–4). Very close parallels are to be found in Pylos, Tomb E8 (Blegen, 1973: figs 249.19 and 249.21).

Professor N. Ceka has reported some fragments of Mycenaean pottery imports and some Mycenaean pottery imitations from the open settlement at Belsh in the Shkumbin Valley, Elbasan (1972: 9). I have not seen these fragments, but judging from the drawings in his report they seem to be of LHIIIB–C date, a period when Mycenaean pottery imitations became common in the periphery of the Mycenaean world. This date was accepted by Professor Harding, who saw the fragments while participating the First Colloquium of Illyrian Studies in Tiranë in 1972 (1984: 239).

It is Professor Harding who, in his *The Mycenaeans and Europe* mentions some possible Mycenaean sherds with painted bands from the Gramsh area, seen in the Elbasan Museum in 1972 (1984: 239).

Another British scholar, Professor Hammond, who before the World War II visited southern Albania, picked up two Mycenaean sherds from the unenclosed settlement at Triport (described above), and reported them in *Epirus* (1967a: 313) as LHIII in date, but without providing any illustrations. As mentioned above, excavations were caried out in the late 1970s on this settlement, but no other Mycenaean pottery was reported.

The painted *lekythos* (Fig. 6.2) comes from Grave 14 of the Tumulus I at Bajkaj. It has appeared in several publications with various descriptions and dates (Budina, 1971: 60; Korkuti, 1982: 249; Eggebrecht, 1988: 208, fig. 60). The *lekythos* is about 0.25 m high, with a high flat base, a slightly ovoid body, and a small handle attached somewhere in the middle of the neck and over the shoulder. It is painted, not very carefully, in the lower part of the body. Over a clumsily painted band on the upper part of the body (on the shoulder) are seven groups, containing six hand-drawn semicircles each. A relatively wide painted band is found where the neck is attached to the body and there are some traces of decoration on the handle.

The *lekythos* from Bajkaj has a Mycenaean shape (later ones have a more ovoid body), which is evident at Pylos, Tombs E8 and K1 (Blegen, 1973: figs 249.16 and 274.10), Kalapodi layer 16 (Jacob-Felsch, 1987: 32, fig. 55), and Kephallenia (Wardle, 1972: figs 100.1677 and 100.1681). The closest parallel to this *lekythos* appears in Ken Wardle's thesis (1972: fig. 100.1681). The differences to that from Kephallenia are unimportant, such as the treatment of the lower part of the body and the shape of the handle. It seems very likely that both examples are of LHIIIC Late date.

Dr Bodinaku's excavations in some tumuli in the Aous Valley have provided three examples of Mycenaean pottery (1981: 243-9). A narrow-necked jug from Grave 103 of the tumulus at Piskovë (Harding, 1984: 239), definitely displays all the characteristics of Achaean Mycenaean pottery. Another narrow-necked jug was discovered in Grave 88 (Fig. 7). This jug seems to have been a local copy of this type of Achaean jug imported here (as the example from Grave 103 shows). It is wheel-thrown but the potter did not seem to be well-trained in reproducing the standard shapes and decoration from Achaean pottery. It is difficult to pin-point where the local centre of pottery production was, but it is likely to have been somewhere in north-west Greece rather than in the Aous Valley. Bodinaku reported

some chance finds from a destroyed tumulus at Këlcyrë on the right bank of the Aous River (1988: 34). He described another narrow-necked wheel-made jug found outside the grave of this tumulus, with traces of decoration on its surface, but not enough to reconstruct the motif, although it appears to be similar to that from Grave 88 at Piskovë. This must be another example of a local production of Mycenaean pottery in north-west Greece.

Some very interesting evidence for Mycenaean pottery imports to south-east Albania has been found in the tumuli cemetery at Barç. About 7% of all the pottery excavated from the two tumuli here is a Mycenaean import or imitation. This is the largest amount of Mycenaean pottery found in any LBA site in southern Albania. The detailed descriptions are as follows:

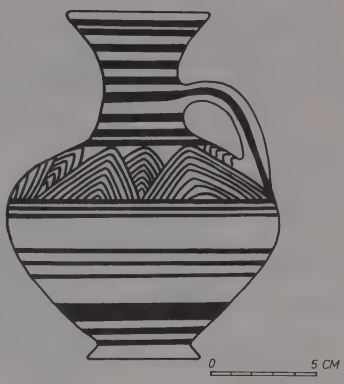

FIG. 7. Mycenaean pottery, narrow-necked jug, Grave 88, Tumulus I, Piskovë (drawn by Ilir Zaloshnja).

Tumulus I

Grave 18. Belly-handled *amphoriskos* (Fig. 8.1). Height 0.114 m; rim diam. 0.059 m; max. diam. 0.118 m. Is complete except for a rim fragment; fairly complete after restoration. The surface is damaged and it is difficult to see the decorative elements. Yellow-brown clay with very occasional sand and mica inclusions. Brown paint.

Flat-topped, everted rim, cylindrical neck. Very biconical shape (max. diameter tends to be as long as the height). High, hollow ring base. Two small semicircular handles, placed horizontally above the max. diameter line.

Outside: painted horizontal bands, combined with thin lines, neatly executed. Handles are half painted.

Inside: plain.

Grave 47. Stirrup-jar (Fig. 8.2). Height 0.176 m; max. diam. 0.143 m; rim diam. 0.039 m. Very fragmentary. Missing one handle and some fragments of the base and body. Restored more that once, now relatively complete. Damaged surface and not all the decorative motifs can be seen. Yellow-brown clay, not very porous. Where it is well preserved, the surface is smooth. Dark brown paint.

Globular body, high, hollow ring base. On the upper part of the false neck and shoulder are two vertical handles.

Outside: lower half of the body is completely painted. Over the max. diameter are three fine horizontal parallel lines, above which, on the shoulder zone, are hatched triangles. The existing handle is decorated with small thick lines. Where the neck meets the body is another thick painted band.

Inside: plain.

Grave 55. Jug with cut-away neck (Fig. 8.3). Height 0.187 m; rim diam. 0.048 m; max.

diam. 0.113 m. Now complete apart from one neck fragment, but was restored on the basis of a small number of fragments. Well preserved. Light brown clay, which has small sand and mica inclusions. Fairly porous. Smooth surface (where it is not damaged). Dark brown paint.

Very unusual shape. High neck, cut. Slightly everted rim. Conical body with pointed base. At the end of the cut-away neck and on the shoulder a vertical handle (grooved in the middle), is carefully placed.

Outside: from the maximum diameter line up to the rim there are three decorated zones of similar width. The upper zone is decorated with zigzag lines linked by small semicircles. Other two zones are decorated with hatched triangles. The lower body, up to the pointed base, is decorated with well-executed bands of similar width. There are some traces of painting on the handle as well.

Inside: plain.

Grave 65. Deep cup (Fig. 9.1). Height 0.074 m; rim diam. 0.093 m. Complete. Very well preserved. Light yellow-brown clay with very small sand inclusions. Well fired. Smooth surface. Dark brown paint.

Globular body, with slightly everted rim, high hollow ring base and two small semicircular handles placed horizontally.

Outside: an ill-executed painting covers the surface including the ring base and the handles.

Inside: unpainted band just below the rim. The rest is painted in the same way as outside.

Grave 149. Two fragments (Figs 9.2, 9.3). Fig. 9.2 is 0.037 x 0.040 m; Fig. 9.3 is 0.070 x 0.040 m. The fragment (Fig. 9.2) consists of three pieces. Light yellow-brown clay in both cases, well fired, with very small sand inclusions. Smooth surfaces, where not damaged. Both painted dark brown.

Judging by the shape and decoration, and thin profiles, it seems likely that both fragments represent closed shapes.

Outside: both are painted with concentric semicircles, with the outer fringed. On one fragment (Fig. 9.2) there are groups of fine lines under the decorated zone.

Inside: plain.

Tumulus II

Grave 6. *Kylix* (Fig. 8.4). Height 0.113 m; rim diam. 0.104 m. Incomplete; missing part of the handle and base. Dark brown clay, with small sand inclusions. Smooth surface. Painted dark brown. Body is conical, with high cylindrical base and two vertical handles rising above the rim.

Outside: entire body is painted with a light colour, generally damaged. The middle of the body is decorated with a foliate band.

Inside: walls painted light brown. Two bands in the middle.

Grave 11. Belly-handled *amphoriskos* (Fig. 8.5). Height 0.187 m; rim diam. 0.095 m; max.

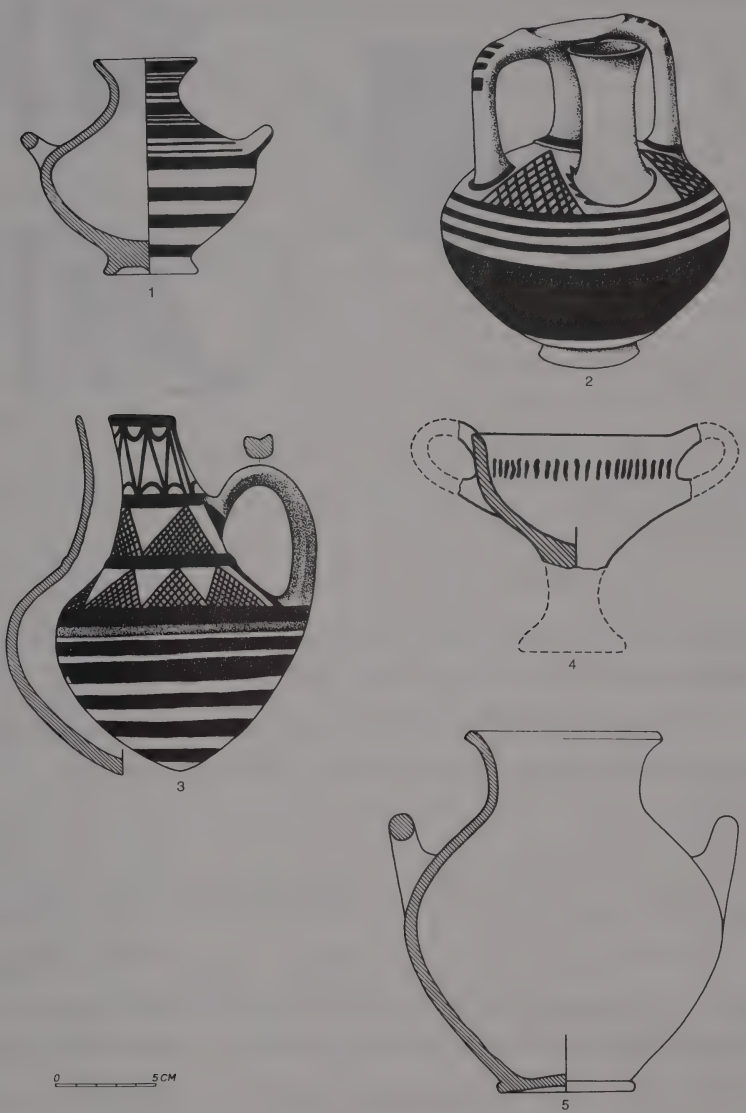

FIG. 8. Mycenaean pottery from Barç.
8.1 Belly-handled *amphoriskos*, Grave 18, Tumulus I.
8.2 Stirrup-jar, Grave 47, Tumulus I.
8.3 Jug with cut-away neck, Grave 55, Tumulus I.
8.4 *Kylix*, Grave 6, Tumulus II.
8.5 Belly-handled *amphoriskos*, Grave 11, Tumulus II. (Drawn by Ilir Zaloshnja.)

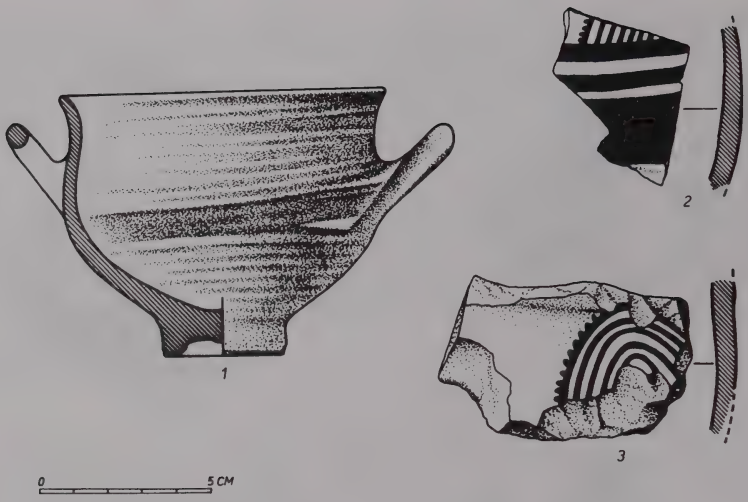

FIG. 9. Mycenaean pottery from Barç.
9.1 Cup, Grave 65, Tumulus I. 9.2 and 9.3 Decorated sherds, Grave 149, Tumulus I.
(Drawn by Ilir Zaloshnja.)

diam. 0.161 m. Very fragmentary. Fairly complete after restoration. Light brown clay, well fired, containing sand and mica. The surface is not very smooth and there are some small holes.

Globular body with a high cylindrical neck and slightly everted rim, hollow ring base and two small semicircular handles placed horizontally over the belly.

Outside: plain.

Inside: plain.

Most of the pottery from the graves of Tumuli I and II at Barç is obviously dated to the Late Mycenaean Period. The *amphoriskos* of Grave 18, Tumulus I, has a characteristic shape of LHIIIC. Late *amphoriskoi* are known from Kangadhi at Achaea (Papadopoulos, 1979: 211, serial no. 498, figs 158h and 248g) and Kephallenia (Wardle, 1972: fig. 94.1667). The decoration also supports this date as it possesses one of the features of LHIIIC Late patterns: the break up of the groups of fine lines (Mountjoy, 1986: 5). I consider it to be an Achaean import.

The stirrup-jar from Grave 47 of the same tumulus has a round shape, typical of the LHIIIC Late or even Submycenaean, with very good parallels from Kephallenia (Wardle, 1972: figs 104.1538, 105.1343). Lord William Taylour reports a similar shape from Campania in South Italy (1958: 174, no. 2, pl. 17.3). The stirrup-jars from both Campania and Barç may be imports from Kephallenia. The Barç example has excellent parallels from Kephallenia for its decoration as well (Wardle, 1972: fig. 104.1538).

The deep cup from Grave 65, Tumulus I, with its ill-executed painting and unpainted band inside, below the rim, suggests a late date (LHIIIC). I cannot be sure where it comes from, but it is very likely to be an Achaean production (it is in good condition and the paint is unusually well preserved for a product from Kephallenia).

The fragments from Grave 149 display features from Achaean pottery of LHIIIC Late date as well (i.e. fringed semicircles and fine parallel lines). This pattern is common at Thermon (Wardle, 1972: figs 41.81, 49.134; Papadopoulos, 1979: 176, fig. 200).

The cut-away necked jug of Grave 55, Tumulus I, is more problematic. Its fabric and decoration may be of LHIII date (suggested by its context), but its shape is unusual in north-west Greece or the north-west Peloponnese where we find the closest parallels for the other Mycenaean imports at Barç. It seems that this may represent an eastern influence in this part of southern Albania, but at the moment I cannot be more specific.

The *kylix* from Grave 6, Tumulus II, has a thick profile and base and is of poor fabric and quality. Even though its decoration suggests a LHII A2–B date, it is not a classical product and I consider it to be much later, coming from a secondary centre of Mycenaean pottery production somewhere in central Macedonia or western Thessaly.

The *amphoriskos* from Grave 11, Tumulus II, has a very late shape (LHIIIC Late-Submycenaean), probably with parallels at Thebes (I owe this suggestion to Dr P. Mountjoy).

At Tren, on the eastern edge of the Korçë Basin, there are some indications of Mycenaean pottery. From the publication of Korkuti (1971: pl. XII 7.9), Professor Harding suggested that two fragments of a deep bowl and a monochrome *kylix* were Mycenaean (1984: 239). Judging from the publication, I would prefer a LHIIIB date for the *kylix*, because of its monochrome painting and very high base. One can suggest other examples of Mycenaean pottery from Korkuti's illustrations, but there must be doubts without a careful study of the fragments themselves.

Within the Korçë Basin, at Sovjan, the excavations have brought to light some other Mycenaean fragments (I have to thank Professor Prendi for showing me drawings of these). One is painted with fringed semicircles over fine parallel lines, and appears to be a very good candidate for being an Achaean import of LHIIIC Late date. Unfortunately the other fragments have incomplete motifs and cannot be accurately dated.

CONCLUSION

I have examined some classes of objects of the Middle and Late Bronze Ages from southern Albania: Grey fabric pottery, some Middle Bronze Age bronzes and Mycenaean pottery.

Grey fabric pottery is fairly common and has a wide distribution. It generally reproduced Grey Minyan pottery shapes (except for the carinated shapes that are present only in small numbers), but the technology differed. The distribution of this kind of pottery in southern Albania is not different from that in north-west Greece or western Thessaly. The same state of affairs seems to happen with various kinds of bronzes of the MBA coming from tumuli in this part of Albania.

There seem to have been very close contacts between southern Albania, the Ionian islands and Thessaly during this period. The appearance of some Cretan bronzes, together with

other artefact types, confirms that this territory was part of the Aegean world and the weapons from tumuli suggest a degree of Aegean acculturation, influencing the organization of society.

The appearance of the first Mycenaean import to southern Albania (the *vapheio* cup from Pazhok), the first in the whole of north-west Greece, is not surprising and indeed may be incidental. Nevertheless, during that period, Mycenaean pottery did not occur until the very late Mycenaean period. At the same time, Thessaly and central Macedonia become increasingly Mycenaean. The situation in the MBA is contradicted by what appears later. An explanation for this cannot easily be found and needs more evidence from both southern Albania and north-west Greece. More careful studies, including modern analyses, particularly of selected classes of materials, are needed to help in answering these questions.

Abstract

This article presents some general information on the present state of research on the Middle and Late Bronze Age in southern Albania: evidence from the settlements, houses, tombs and burial customs; a re-consideration of the Grey fabric pottery of the Middle Bronze Age and of some bronzes including long swords, daggers, spearheads, and knives; a description and re-consideration of Mycenaean pottery from southern Albania (Barç, Tren, Pazhok, Belsh, Bajkaj, Piskovë); with some conclusions and interpretations.

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Mountains, miniatures, ancestors, and fertility. The meaning of a Late Horizon offering in a Middle Horizon structure in Peru

by FRANK MEDDENS*

INTRODUCTION

Between 1978 and the end of 1982 the Chicha Project carried out a series of archaeological explorations in the Chicha/Soras valley along the departmental borders of Ayacucho with Apurimac, in Peru. This article places the find of a series of small and miniature pottery vessels dating to the Late Horizon in an interpretative framework, using historical and ethnographic information.

Documentary evidence suggests that at the time of the Inca conquest of the area, the Ayacucho or Lucanas side of the Chicha river valley lay in the territory of the Soras group, whereas the Apurimac side was part of lands controlled by the Chanka. The Chanka and Soras both formed part of an affiliation of ethnic groups currently defined as the Chanka Confederation (Lumbreras, 1974: 180, 198–200; Anders, 1986: 782–8).

The Soras area, in Early Colonial times, was made up of three parts, Anansoras, Lurinsoras and Chalcos. The settlement of Soras was the principal town of the region. Three languages were current in the territory, Quechua, Aymara and Hahuasimi; the latter may be translated as the language of the ancestors (Jimenez de la Espada, 1881: Vol. 1, 169–71).

A LATE HORIZON SITE

An archaeological site called Chiqna Jota, with evidence of a significant Late Horizon component, is situated on the margins of the hamlet of Chicha. It is located immediately to the south of the village, at an altitude of 3450 m above sea-level. There are some 200 standing structures present, covering 9 ha. (Fig. 1). It was occupied from Middle Horizon Epoch 2 through the Late Horizon, and seems likely to have been abandoned within 30 years of the conquest, since it does not appear to be mentioned in the Albornoz *visita* (Millones, 1971), which took place before the re-organization of settlements in the area under the Viceroy Don Francisco de Toledo.[1] It is situated near the confluence of the Pachachaca with the Chicha river. Circular, square, sub-rectangular and rectangular buildings occur, with both rounded and squared corners. The site was arranged around three sunken plazas and

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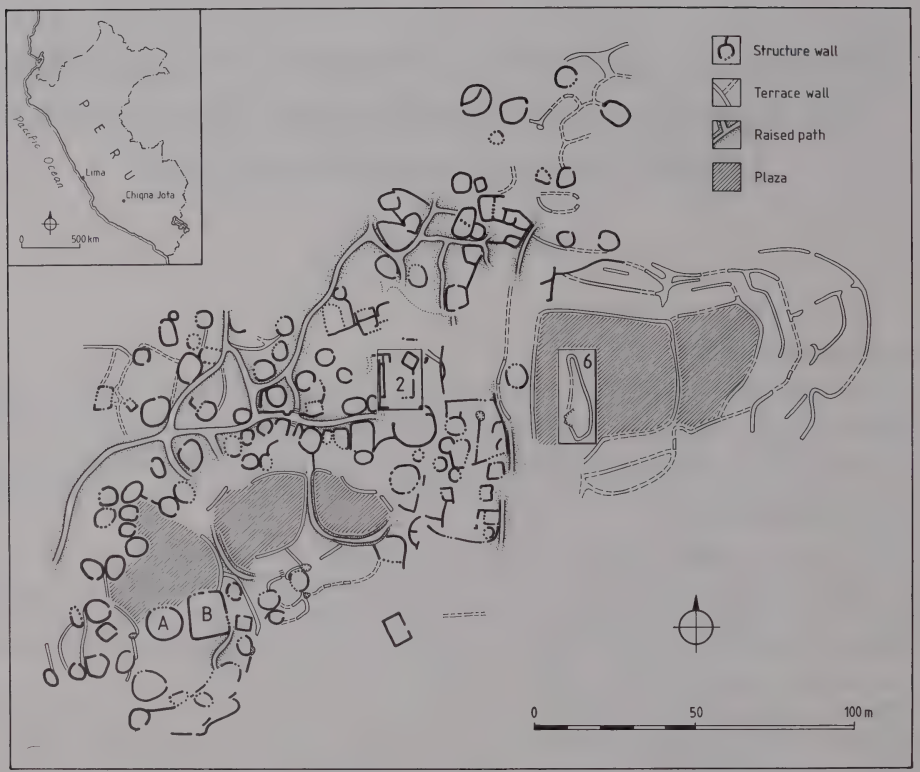

FIG. 1. Partial plan of Chiqna Jota.

two hills. Early in the Late Intermediate Period a series of raised pathways were constructed dividing the site into distinct units. During the Late Horizon, two large buildings, one circular with a diameter of 12 m^2 (A on Fig. 1), and a rectangular one with rounded corners measuring 13 m x 15 m^2 (B on Fig. 1), were built, side by side, on the south side of the westernmost plaza. Both were constructed of roughly dressed fieldstone. A number of other buildings were added to the site, some built of polygonal ashlar and some with ashlar facing on fieldstone. Several had red painted clay plaster on the interior walls. Some staircases and doorways were blocked off and the internal organization of the site was changed considerably.

One of the features excavated here (excavation unit 2; no. 2 on Fig. 1; Figs 2 and 3) consisted of a narrow corridor or long rectangular room subdivided into small cellular units. It was located overlooking the principal plaza from the western side (Fig. 1). This plaza was surrounded on all sides by terracing. It was open on its eastern side, where occupation ceased, and a steep slope descends to the floodplain of the Chicha river.

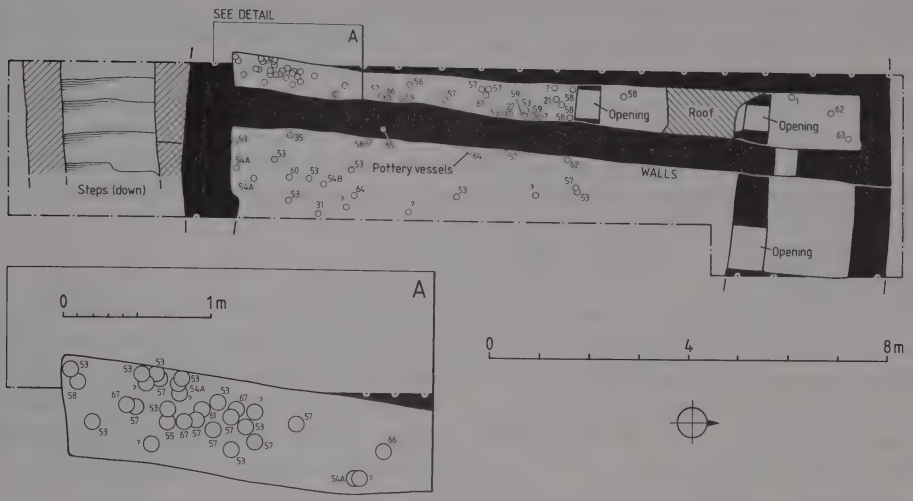

FIG. 2. A plan of the structure in excavation unit 2 with associated distribution of pottery vessels.

A structure situated on the western margin of this plaza seems likely to have represented an *ushnu* (*ushnu* as used here means a platform on which the local ruler is located on ceremonial occasions). It consisted of a stepped, irregular platform, originally probably sub-rectangular in shape, measuring 26 by 8 m, rising up to 2.6 m above the ground surface (no. 6 on Fig. 1).

The corridor (Figs 2 and 3) was constructed during Middle Horizon Epoch 2 (Meddens, 1985: 139–43). It was built into the hill slope, following the contour and it measured internally approximately 12.7 m in length, tapering from 1.10 m to 0.58 m in width at floor level. The wall thickness varied from 0.75 m to 0.90 m and the walls were constructed of roughly dressed fieldstone covered with a clay plaster. The foundations were cut into the underlying natural volcanic tuff and the walls in section were roughly trapezoidal. The west wall of the corridor constituted the retaining wall of the terrace ascending behind it. The east wall is parallel to the terrace wall, and both were bonded into end walls to the south and north. Two internal partition walls abutted the east and west wall of the structure. Each of these had a small window or entrance: the one where accurate measurements could be made was 0.52 m wide by 0.76 m high. The roof construction of the structure consisted of rows of lintels placed parallel on corbels protruding inward from both the east and west walls. Sometime during Middle Horizon Epoch 2 a small additional chamber was added onto the side of the corridor, on the north-east limit of the structure. This gave entrance to the corridor through a window or doorway measuring 0.48 m wide by 0.82 m high. Immediately beyond the southern end of the corridor a staircase allows access to the top of the terrace against which the corridor is built. The construction of these stairs consists of three parallel

abutting walls which together with the top of the terrace wall make up the steps. The first three steps are contained between two walls set at right angles from them. These two walls are not bonded into the steps, the terrace wall or the wall of the corridor (Figs 2 and 3).

The south wall of the corridor is aligned on an east-west orientation. The east section of this wall continued well beyond the limit of excavation, and its eastern end was not defined in excavation. The position of various other wall remains further east suggest that the corridor may originally have been part of a significantly larger complex (Figs 1 and 3).

THE OFFERING

The corridor was in a state of disuse and partial collapse when, during the Late Horizon, at least 84[3] miniature[4] and small pottery vessels were placed inside it. For the purposes of this paper all complete and near complete small and miniature vessels directly associated with the structure have been considered. This includes those that had been re-deposited in disturbances to parts of the gallery in periods later than its final collapse (possibly as a result of treasure hunting activities or a Colonial period attempt to destroy a *huaca*, any material thing which manifested the sacred and divine).

The classification of the vessel types is part of a comprehensive typological framework covering a number of periods and sites in the Chicha/Soras valley (Meddens, 1985), which is why the numbers identifying the vessel shapes are not consecutive and may appear high.

Other pottery from relevant contexts consists of small numbers of sherds of Late Horizon date with some residual material from earlier periods, neither of which will be considered in any detail in the rest of this paper. None of the finds from any of the gallery contexts associated with the assemblage of miniature and small vessels post-dates the Late Horizon. That is not to say that the final destruction of the corridor complex could not have occurred early in the Colonial period. It does however place the offering itself firmly within the Late Horizon, and at a time after the Inca conquest of the area.

The vessel shapes

The description of the vessel shape categories is based entirely on the sample from the corridor complex. They have been ordered in the text according to their relative frequency in the assemblage (and by type of shape in Figures 4 and 5), starting with the most common shape, although 11.9% could not be identified by vessel shape.

Vessel shape 53, a small jug with a vertically placed handle positioned from the vessel lip to body (Fig. 4.1), forms the most frequent category (21.4% of the assemblage). Diameters range from 4 to 10 cm with 87.5% from 4 to 6 cm (mean 5.38, standard deviation 1.7). Vessel heights range from 5 to 13 cm, with 75.05% from 6 to 8 cm (mean 8.3, standard deviation 2.07). The surface finish varies from coarse (20%) to medium (80%), and the surface colours are either black, dark brown, brown or orange, with 86.7% being brown or dark brown.

Vessel shape 57, a small short-necked bottle with two or three vertical pierced lug handles on the body (Fig. 4.6) constitutes 15.5% of the assemblage. Rim diameters range from 3 to

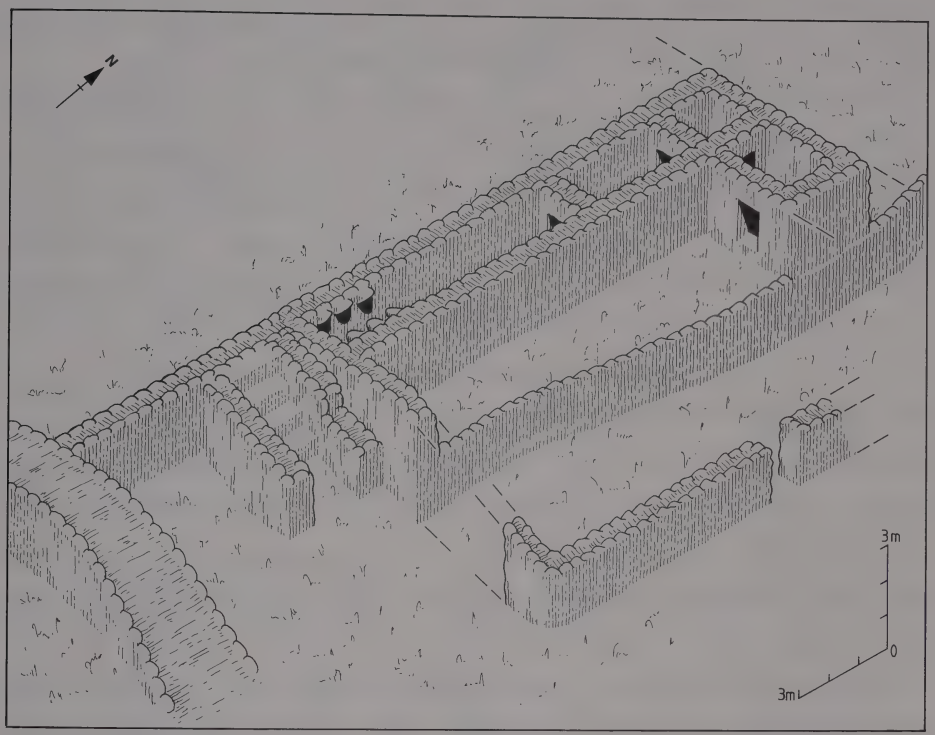

FIG. 3. Isometric reconstruction of the structure in excavation unit 2 showing possible larger complex.

5 cm, (mean 3.94, standard deviation 0.81). Vessel heights range from 6 to 10 cm (mean 7.95, standard deviation 1.31). Surface treatment ranges from fine to medium and coarse, and the surface colour is dark grey, brown or dark brown with 75% dark brown.

Vessel shape 58, a small squat-necked jar, with flaring rim, bulbous body, flat to rounded base and vertically placed strap handle on the body (Figs 4.4 and 4.5), makes up 7.14% of the assemblage. Rim diameters range from 3 to 8 cm (mean 5.83, standard deviation 1.57). Vessel heights span 8 to 10 cm, and 66.7% are 8 cm high (mean 8.03, standard deviation 0.74). Surface treatment ranges from coarse, medium to a fine finish. The surface colour is either dark grey, dark brown or brown, with 87.5% brown to dark brown. This vessel shape is similar to one found in an Ica tomb group, dating to the Late Horizon (Menzel, 1976: pl. 13, 167–8) in a local Inca style (ibid. 75).

Vessel shape 54A, a small jug with a turned-over rim, has a strongly tapering body from rim to base and a flat base. The handle is vertically placed and positioned on the body (Fig. 5.15). This group accounts for 4.76% of the assemblage. Rim diameters range from 4 to

5 cm (mean 4.6, standard deviation 0.49). Vessel heights extent from 8 to 12 cm (mean 9.9, standard deviation 1.56). The surface finish in all cases is medium, and the surface colour is either purple, dark brown or brown, with 80% brown to dark brown.

Vessel shape 54B is similar to 54A. It is a small jug with a turned-over rim, a strongly tapering body from rim to base, with a flat base, and represents 1.2% of the collection. The handle is vertically placed and runs from lip to body (Fig. 5.16). The rim diameter is 4 cm and vessel height is 12 cm. The surface finish is medium, and surface colour is purple.

Vessel shape 59, a small tapering-necked vessel, has a wider diameter where neck and body join than at the lip (Fig. 4.8). This group forms 4.76% of the pottery. The rim diameters range from 3 to 6 cm (mean 4.33, standard deviation 1.25). The vessel heights extent from 5 to 12 cm (mean 8.1, standard deviation 3.08). The surface finish is either coarse or medium and the surface colour is dark brown.

Vessel shape 64, a miniature short-necked jar (Fig. 4.3), also constitutes 4.76% of the assemblage. The diameters of all examples were 3 cm, and the vessel heights ranged from 5 to 8 cm (mean 5.76, standard deviation 1.22). The surface finish is mostly fine or medium fine with one coarse example and the surface colour is mostly dark brown with one black specimen. This shape is similar to an example from a tomb from Ica of Late Horizon date (Menzel, 1976: pl. 13, 169) in a local Inca style (ibid. 75).

Vessel shape 61, a miniature straight-necked bottle (Figs 4.9a and 4.9b), makes up 3.57% of the collection. Diameters range from 3 to 4 cm (mean 3.5, standard deviation 0.5), and vessel heights from 4 to 9 cm (mean 6.73, standard deviation 1.98). The surface finish is fine and the colour red.

Vessel shape 62, a miniature jar, with a short slightly flaring lip, two vertical pierced lug handles, and a convex base, (Fig. 4.2), represents 3.57% of the assemblage. Diameters range from 3 to 5 cm (mean 4, standard deviation 0.82) and vessel heights range from 6 to 9 cm (mean 7.43, standard deviation 1.20). The surface finish is fine or medium, and the colour is black or orange.

Vessel shape 67 is a round-bodied jar with two and occasionally three handles which, in cases with two, are placed horizontally and where a third occurs it is placed vertically on the top third of the body. The base is pointed and in some examples is concave on the circumference ending in a protruding central point (Fig. 4.7). It makes up 3.57% of the collection. The diameters range from 5 to 9 cm (mean 7, standard deviation 2), and vessel heights from 9 to 14 cm (mean 12.13, standard deviation 2.22). The surface finish is fine or medium and coloured dark brown or black. It is similar to Rowe's vessel shape 'e' (1944: 48). The gallery examples are not decorated, however, and are comparatively small.

Vessel shape 1 is a convex-based open bowl (Fig. 4.11), which constitutes 3.57% of the assemblage. Rim diameters range from 9 to 18 cm (mean 13.33, standard deviation 3.68), and vessel heights from 5 to 7 cm (mean 5.77, standard deviation 3.68). The surface treatment is fine or medium and the surface colour is dark brown, red or orange. The examples of this type vary from small to full-size.

There are two examples (2.4%) of vessel shape 22, a straight-sided, round-based cup with a vertical handle (Fig. 5.18). The diameters range from 5 to 7 cm, and the vessel height in the

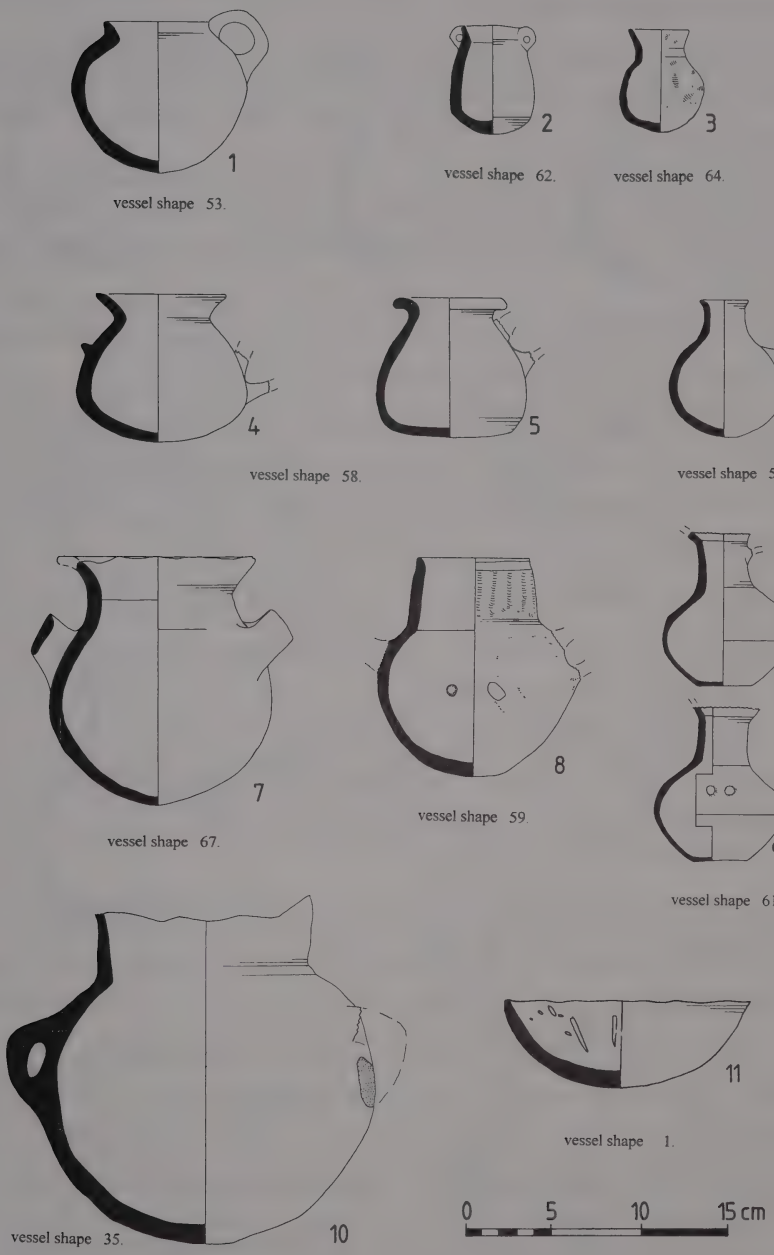

FIG. 4. Miniature and small pottery vessels from the gallery offering.

one case where it could be measured is 7 cm. The surface finish is coarse, and coloured brown.

Vessel shape 69, a short-necked jar with angled body and flat base (Fig. 5.12), is represented by one specimen (1.2%). It had two knobs placed next to each other on the ridge of the neck. The diameter is 9 cm and the vessel height is 13 cm. The surface colour is dark brown and the surface finish is fine.

Vessel shape 66, a jar with a convex body, flat base and gradually flaring neck, with a loop handle from the body to the lip (Fig. 5.17), is represented by one example (1.2%). Neither rim diameter nor vessel height could be measured. The surface colour is orange and the finish is fine.

There is only one specimen (1.2%) of vessel shape 63, a small open bowl, with a single vertical handle from the lip (Fig. 5.23). The diameter of this vessel is 7 cm and the height is 4 cm. The surface colour is dark brown and the surface finish is fine.

There is one example (1.2%) of vessel shape 60, a miniature short-necked jar with a spout on the body and a horizontally placed handle on the opposite side of the vessel (Fig. 5.19). The diameter is 5 cm and height 6 cm. The surface colour is brown and the finish medium.

Vessel shape 56, a miniature incurving bowl, with a vertical handle from lip to body (Fig. 5.21), is represented by one specimen. The diameter is 4 cm and height 6 cm. The surface colour is brown and the finish is coarse.

Vessel shape 55, a small jar with a stirrup handle (Fig. 5.14), is represented by one example (1.2%). The diameter is 4 cm, and height is 10 cm. The surface colour is dark brown, and the surface finish is medium. This type is somewhat similar to Rowe's vessel shape 'd' (ibid. 48) and Julien's shape 3a (Julien, 1990: 31). It clearly is a very poor imitation, lacking the strong inflections of the base present in the Cuzco Inca pieces, without decoration of any kind, and in general being of low quality.

Vessel shape 35, a full-size, simple short-necked jar with a short straight neck, globular body and convex base (Fig. 4.10) is represented by one specimen (1.2%). The diameter cannot be measured but the vessel height is 18 cm. The surface colour is black and the finish is medium.

There is one example (1.2%) of vessel shape 31, a ladle (Fig. 5.22). The diameter is 4 cm and spoon length is 7 cm. The surface colour is brown and surface finish is medium.

Vessel shape 65, represented by one example (1.2%) (Fig. 5.13), consists of a small jug with a vertical handle on the body and a convex base. The diameter is 5 cm and the height 9 cm. The surface finish is medium and the surface colour is brown.

Finally vessel shape 21, the *manca*, is a flat-based bowl with somewhat inwardly inclined vessel walls, a slightly outward projecting rim and flattened lip, and two opposing horizontally positioned handles on the body (Fig. 5.20). It is also represented by a single specimen (1.2%). The diameter is 11 cm and height is 8 cm. The surface colour ranges from brown to black, and the surface finish is medium. This vessel shape is the equivalent of Rowe's shape 'f' (1944: 48) and Julien's shape 6 (1990: 31–2)—again the gallery example is undecorated. This appears to be a relatively common shape in certain assemblages from the Cuzco area (Valcarcel, 1935: II 1/399, V 1/271, VI 1/641, VIII 3/366, 3/365).

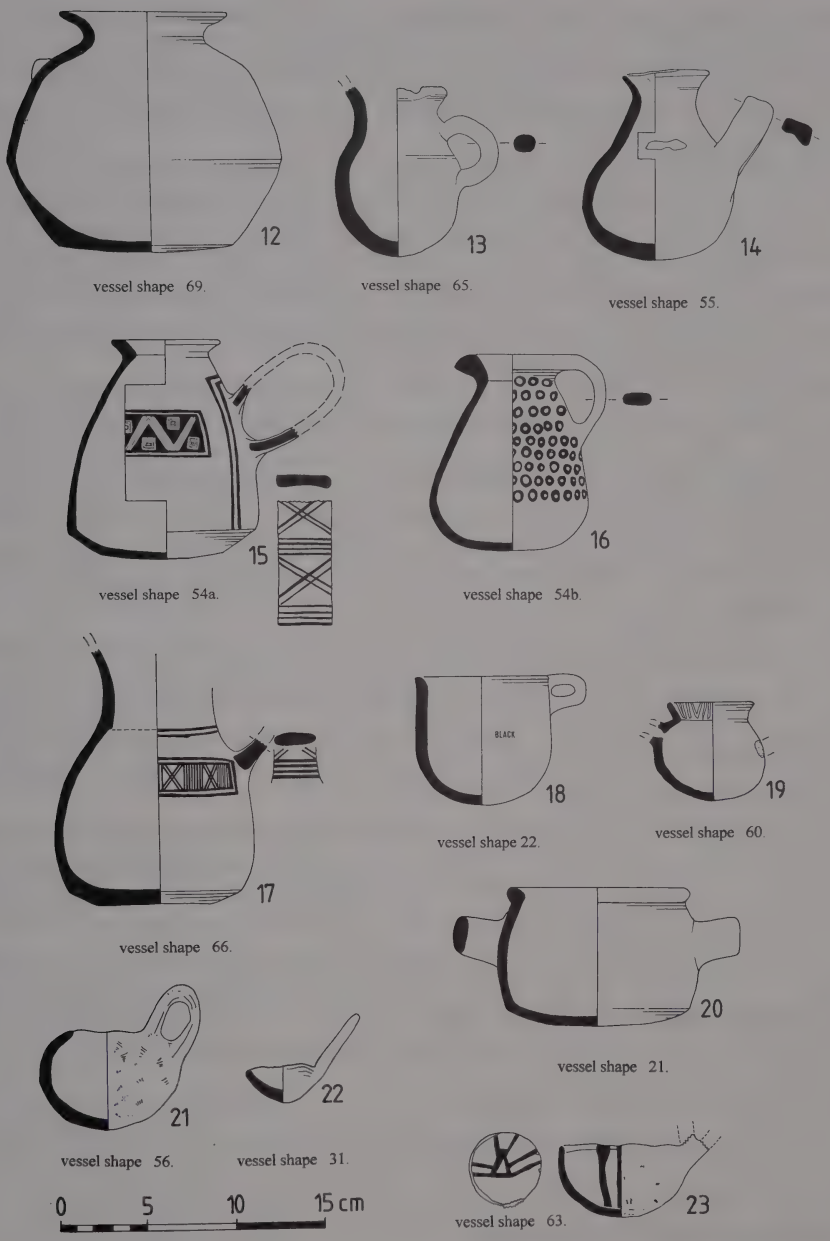

FIG. 5. Miniature and small pottery vessels from the gallery offering.

Decoration

Decorative designs are rare in the assemblage. The most elaborate examples occur on two pieces that are likely to have been imported: vessel shapes 54A and an example of 66. The design configurations are similar to Julien's mode 'F' (1990: 37), which is characterized by bands or outlined designs positioned horizontally on the body and which do not occupy all of the principal decorative space.

A zigzag design with square concentric filler elements similar to the example present on vessel shape 54A (Fig. 5.15) occurs in the Cuzco-Inca styles (Fernandez Baca, 1971: 119, figs 122, 237 and 251). In the case of Chiqna Jota, the section of the vessel wall to which the handle is attached is outlined by a double line motif. A third motif, an outlined band consisting of pairs of crossed lines separated by multiple vertical lines (Fig. 5.15) is used on the handle. The colour combination used here is black, purple and white on an orange base. This design is similar to Julien's mode 'A' (1990: 34).

The latter motif occurs on both the types 54A and 66. It is used in a horizontal outlined band on the principal decorative space and also on the handle on type 66. This vessel has a set of horizontal parallel lines dividing the neck from the body (Fig. 5.17). The colours used here are black, cream and dark brown on a brown base. It is a common design element during the Late Horizon in the Cuzco area (Fernandez Baca, 1971: 96–9; Julien, 1990: 34).

A relatively common decorative feature in the collection consists of a single unoutlined band of one colour around the interior of the lip. This very simple attribute occurs in 20% of the assemblage.

A further design consist of a series of thickly executed small black circles on a deep red base colour covering two thirds of the exterior of the body of the vessel of type 54B (Fig. 5.16). This design does not appear in the Cuzco-Inca style. The vessel shape may represent a local imitation of the Cuzco-Inca ceramic style represented more clearly by vessel shape 54A; the general vessel form is similar although it must be noted that both lip treatment and handle placement are at variance.

The design present on vessel shape 63 consists of a stylised anthropomorphic or zoomorphic design consisting of a figure with a triangular body, a pair of raised arms or legs and a double pair of parallel spread legs. This bowl additionally has an unoutlined band around the interior of the lip (Fig. 5.23). The colours are black and dark brown on brown.

Vessel shape 60 has a band around the interior of the neck with vertical lines occasionally joined at the base like a 'V' (Fig. 5.19). The colours are dark brown and white on a brown base. This design is sloppily executed. Similar designs first occur in Middle Horizon Epoch 2 assemblages from the Chicha/Soras area. They occasionally occur together with sequences of small black or dark brown thick and short vertical lines off the lip.

One example of vessel shape 61 has three horizontal bands on the exterior occupying a third of the exterior surface area. The colours are red, brown and dark brown (Fig. 4.9a and 4.9b).

The range and condition of the pottery

In the assemblage the vessels ranged in height from 4 to 18 cm, with 97% between 4 and

14 cm in height. Their diameters varied from 3 to 18 cm, with 92.35% ranging from 3 to 9 cm (in fact just one vessel was over 13 cm in diameter). Of the assemblage 11.9% could not be classified by shape. Of the identifiable types only 10.7% were shapes other than variations of necked jars and jugs. Twenty-two distinct categories and sub-categories of vessel shapes were distinguished (Figs 4 and 5). Among these vessel shapes, six can be defined as true miniatures (Figs 4.3, 4.6, 4.7, 5.19, 5.21 and 5.22), as full-size examples occur in other Late Horizon assemblages from the Chicha/Soras area. The assemblage was made up of Inca (3.57%), local Inca imitations (17.9%) and Soras-style vessels (76.6%).

The Soras-style pottery is characterized by smudged yellow-brown and dark brown wares. Most is not decorated although among the full-sized examples encountered elsewhere, very roughly made plastic decoration occurs, as do a limited range of smudged, sloppily executed geometric designs. The plastic designs consist predominantly of roughly modelled faces with exaggerated features, usually with enlarged lower lips and/or chins. Most of this pottery is badly modelled and much of it is over-fired. In some assemblages up to 10% of the material was cindered, had vitrified surfaces and was so hard it could be classified as a proto-stoneware.

Much of the assemblage was not in pristine condition when buried: 64.3% was damaged, chipped, cracked, or had a handle missing (Figs 4.4, 4.5, 4.7, 4.8, 4.9, 4.10, 5.13, 5.15, 5.17, 5.19 and 5.23), 5.95% had holes made after firing (Figs 4.8, 4.9b and 5.14), 15.46% was eroded or showed evidence of wear and 2.38% had been repaired in antiquity. In addition 10.71% had smudges of charcoal adhering to the surface. It therefore appears likely that much of it had been used in an alternative role prior to its deposition in the gallery context. The fact that many of these vessels had been used in a fire suggests that they may have been employed in some form of cooking procedure, but because of the small size of the pots, are unlikely to have been particularly practical. The purpose of the activity may have been for ritual food or *chicha* (a form of maize beer) preparation. Indeed vessel shape 67 (Fig. 4.7) may well be a miniature form of a much larger vessel used in the preparation of *chicha* (Julien, 1990: 39). Considering the poor quality of much of this material the two repaired pieces in the assemblage are noteworthy. It suggests that this pottery had some inherent quality and value other than that apparent from its appearance.

The similarity of a few of the pieces to some from a Late Horizon burial from the Ica area, executed in a local Inca style, is of interest. A number of Ica/Chincha style vessels occur in privately held pottery collections in the Chicha/Soras valley which allegedly came from graves in the vicinity (Meddens, 1985: 228, 394, fig. 158). De Cieza de Leon mentions Chincha raids in the Soras domain (de Cieza de Leon, 1947: 423–4).

Vessel distribution and context

The excavated area can be divided into five distinct spaces. These consist of four conjoining cells and a space which has been defined as outside of, or exterior to the structure. It has been explained above that the structure may be part of a larger complex and the definition of outside or exterior may therefore be a fallacious one. In looking at the distribution of the vessels it is clear that the greatest concentration of pottery is on the southern side of the

excavated area (Fig. 2) with 94.7% of the vessels being located here, both in the innermost cell and the outside area. One cell, the north-east one, contained two vessels (2.5%) which were found broken, in re-deposited contexts, during the processing and analysis of the material. The north-west one contained three vessels (or 3.8%), all of which were found *in situ*. The central cell held one pot (1.3%) which was found in excavation, and a further broken one (1.3%) which turned up during processing and analysis; both these vessels were from re-deposited contexts. A total of 49 vessels or 62.0% of the total were found inside the third, innermost chamber of the west corridor; all of these were found *in situ*. From the exterior space 23 vessels, representing 29.1% of the total, were recovered (Fig. 2); 13% of these were found *in situ*, with the remainder from disturbed contexts. Those vessels from the assemblage not accounted for in this spatial analysis consisted of fragmented pieces, parts of which were recovered from more then one possible location, or vessels which were from such disturbed contexts that it was judged imprudent to allocate a precise position.

The largest concentration of pots were stacked several layers deep at the narrow and dark southern end of the innermost chamber (Plate 1). Admittedly the central cell of the western corridor was not fully excavated, but considering its size, and the fact that both vessels recovered from this cell were from re-deposited contexts, it seems unlikely that full excavation would have changed the picture significantly.

If we ignore for the moment that 87% of the vessels found in the exterior area were from disturbed contexts and look at the distribution of vessel types between this space and the innermost cell, there do not appear to be significant differences in the types represented (Fig. 2). Vessel shape 53 in both spaces constitutes a common shape, 20.8% and 30.4% respectively, as does vessel shape 57 which makes up for 22.9% and 8.7% of these assemblages respectively.

If we take account of the fact that 87% of the pots had been re-deposited in the outer area, the concentration of vessels in the innermost cell becomes even more pertinent. It seems more than likely that the positioning of the majority of the pottery in the innermost cell was related to this being the most confined, inaccessible, and least public part of the structure. The use of an 'inner sanctum' for the most important part of a building of religious significance is certainly not unique in Inca usage. When excavated this pottery lay in a matrix of very fine silt, which is likely to have been a wind-borne deposit (H. Keeley, personal communication, 1982). If this was indeed the case it suggests a further period of disuse of the structure before its final collapse. A small pair of copper tweezers were associated with one of the vessels, a small imitation Inca-style vessel (Fig. 5.20). Tweezers of this kind would have been used for the removal of facial hair. Hair, particularly from the eyelashes, was commonly offered to the gods by the poorer members of society, who could not afford to devote anything else (Cobo, 1895: Vol. IV, 84). The presence of the tweezers in this context is therefore of interest, particularly in relation to the poor quality of the offering as a whole. Metal tweezers would not be associated with the poorest members of society however. A few fragments of camelid bone were also found. The nature and condition of this bone material does not suggest that one or more animals were specifically slaughtered for the occasion.

PLATE 1. Pottery vessels in the southern end of the innermost chamber during excavation.

The pottery was especially gathered for the event. The low quality of the wares as well as the absence of sea shells, textiles, precious metals, or other high-quality items known to have been used in the context of offerings elsewhere at this time, suggests that this contribution to religious well-being was made by a relatively low stratum of society. It must be said however that as this offering appears to have been a one-off, and because the pottery seems to have had some inherent quality which made it worth offering, it is difficult to make categorical statements about the status of the people involved in producing it.

ETHNOGRAPHIC COMPARISONS

The use of miniatures in Andean religious ritual is known in both prehispanic times and in the more recent past. Most of the current use of miniature objects cannot convincingly be traced back into antiquity. That is not to say that the body of evidence for the Andean area does not point to a long tradition in the use of miniature and small objects, including pottery in a ritual and religious context. In order to get some idea of the nature of this tradition it is useful to discuss examples of relatively recent date as well as examples from the distant past. It is in this tradition that the Chiqna Jota offering can best be viewed.

In Bolivia the Aymara have a good luck spirit known as 'Ekako'. The Ekako is associated with miniature objects, which include miniature pottery in the urban manifestation of its

cult. This spirit is connected with sex, fertility and prosperity. Its cult is particularly strong in La Paz, Cochabamba, and Oruru during annual fairs called *alcacitas*. During this festival a great deal of sexual licentiousness tends to take place on the part of the younger Aymara (Tschopic, 1946: 560, 566–8).

In a Bolivian village south-west of La Paz the planting of crops is imitated using miniature ploughs as part of a fertility ritual, and near Sabaya miniature corrals, fields and houses are used. These ceremonies are associated with mountain gods (Reinhard, 1987: 58). In the Cuzco area miniature houses, herds and corrals are used in pilgrimages to Qoyllur Rit'i. Here too the underlying reason appears to be a material benefit in terms of an increase in herd size or the acquisition of a new house (Sallnow, 1987: 189–90, 201–2). There also appears to be a connection with mountain worship as the shrines related to these pilgrimages are associated with specific mountain peaks which are the major *apus* of the area (ibid. 213–14).

In the community of Chuschi, in the Rio Pampas area, at the times the gods are hungry and require offerings, the *illas* are said to walk. *Illas* are small figures of stone of a sacred nature; they represent items like maize, cheese, llamas, alpacas, corals, sheep, cattle and even humans and geographical locations. These images signify abundance, increased productivity and fertility. They are supposed to be given by the *wamanis* or mountain deities to favoured families (Isbell, 1974: 117–18).

In Raqchi on the night of 24 August, members of the community sit in the corral with figures of sheep and bulls, as well as *trago* and *chicha* (cane spirit and maize beer) 'ovejakuna allin purinayac' (in order that the sheep should walk well). They also bury miniature pots during this night under a stone by the corral; these are filled with *chicha*, *coca* and wine. This offering and the related ceremony are directed to maintaining the health of the animals, but they are also meant to protect the household, and one informant attributed his renewed success with large pottery firings to having performed this ritual (B. Sillar, personal communication 1992).

In Qhunqhu Millumi, in Bolivia, animals for the christmas *challa* (fair) are modelled from clay collected from sources located in the high pasture of the llamas, and the catechist also blesses these figurines with holy water. These models may become aspects of the camelids by this ritual and be involved in protection of the herds.

Also in Bolivia a small bundle is sometimes tied to the main rafter of a new house by the owner, or better still by the *compadre* (godfather and father of a child, used each with respect to the other). This bundle comprises half a dozen miniature pots and bottles filled with *chicha*, wine, alcohol, maize, chilli, *chuno* (freeze-dried potato), llama fat and *coca*. This is seen as an offering to the 'Pachamama' (Earth Mother) to ensure the well-being and fertility of the household (B. Sillar, personal communication, 1992).

Small and miniature vessels also occur as childrens' toys and models in many Peruvian and Bolivian potters' communities (G. Banks, personal communication, 1992). Miniatures and small pots may obtain some of their power from their association with children, children being the product of fertility (B. Sillar, personal communication, 1992). Children are known, from chronicle sources, to have constituted an important means of communication

with the *huacas* (types of Andean gods, spirits, or sacred beings, frequently taking the form of stones, mountains, lakes, springs etc), *malquis* (ancestor mummies and a type of *huaca*) and *apus*. They were commonly used in blood sacrifices such as the Capac Hucha ritual (in which children were sacrificed). Childrens' toys being imbued with a special power as a result of their close association with children should therefore be considered as a possibility.

CHRONICLE AND ARCHIVAL COMPARISONS. RELIGIOUS BELIEFS IN THE ANDEAN AREA

Religious beliefs formed an all-pervading element of prehispanic life in Peru. *Huacas* were ever present, at a personal and household level, for the *ayllu* (the basic kin unit of Andean social structure, endogamous lineage claiming descent from a common ancestor), for the ethnic group and at a state level. *Huacas* and ancestor worship in terms of *malquis* form an inseparable and integrated structure of shrines, culture heroes, deified forebears and deities. Indeed in many Andean origin myths, founding ancestors are transformed into stones or *guancas* and some into mountains or *wamanis*. Besides being a type of *huaca, guancas* were landmarks indicating possession of space (Rostworowski de Diez Canseco, 1988: 33). *Wamanis* are not only the mountains themselves but can also transform into anthropomorphic beings living inside them (Ansion, 1987: 115–44), a belief which is also suggested in some of Guaman Poma's illustrations as being current in Early Colonial times (1980: 238–46). Portable transformations also existed (Cobo, 1892: Vol. III, 324–5; Guaman Poma, 1980) which would frequently be taken into battle in order to ensure victory. Idols which were representations of non-portable deities, constituting these gods themselves, were common and also included, besides mountains and rivers, celestial phenomena (Cobo, 1892: Vol. III, 323–31).

The *wamani* embodies two major functions; it has a creative and procreative role and an ordering and structuring role (Martinez, 1983: 95–105; Anders, 1986: 761), the first instance being associated with fertility and the second with protection and conservation of the structure of society. They have both malevolent and benevolent sides and a hierachical ranking of *wamanis* is known to have existed in the past as well as in the present (ibid. 761–7). *Wamani* and ancestor worship were closely linked. The *malquis* were intermediaries between the living and the *wamanis*, particularly in issues relating to water and fertility. More recent beliefs have been recorded in which the *wamanis* are seen as transformations of dead ancestors (Gonzalez Carre and Rivera Pineda, 1983: 26–8; Anders, 1986: 760).

Offerings to the *wamanis* are frequently made into holes in the ground which are often stone lined. These holes are thought to connect to the interior of the earth, 'Uku Pacha', and through this to the *malquis* and also the *wamani* itself. This practice appears to go back at least to the sixteenth century and is probably considerably older (ibid. 768, 790–1).

All aspects of social, economic and political reality were codified and part of the religious framework. In the economic sense religion was part of the framework of reciprocity which ensured that the *huacas* provided high productivity in agriculture as well as other facets of economic life.

That the structure was dynamic and adaptable to changing circumstance is demonstrated by the incorporation of the *huacas* of conquered nations into its pantheon, as well as the restructuring of the system to accommodate much larger political units, as appears to have happened during the course of the Inca expansion. In this respect it should also be noted that both the more important *huacas* of the conquered nations as well as more prominent members of their *kuracas'* (rulers) households were taken to Cuzco (Garcilaso de la Vega, 1723: 144–5). There both could, in a very real sense, be kept hostage and coerced into the service of the Inca state, the former being worshipped within the state system and the latter being educated in the ways of the Incas.

Within the context of prehispanic administrations, authority was legitimized by a ritualised manipulation of the religious beliefs and concepts of society. It will be argued that the miniature and small pottery, which during the Late Horizon had been placed inside a structure of considerable antiquity, can be related to the ritual maintenance of fertility and possibly to ancestor and *wamani* worship.

BELIEFS OF THE SORAS

The Soras worshipped a mountain called Carahuaraso, (Jimenez de la Espada, 1881: Vol. 1, 172). Carahuaraso appears to have constituted a major mountain deity or *wamani* in this part of the Andes (ibid. 1881:Vol. 1, 172; Millones, 1971: 0/10, 2/52–3, *passim*; Guaman Poma, 1980: 248), although Auqui Uscuntay, a rock on top of a mountain by that name, has also been mentioned as being their principal *huaca* (Duvoils, 1967: 28). The Soras were said to have descended from a lake called Choclo Cocha, which was their *pacarina* (place of origin, see below for more detail) (ibid. 20). The Carahuaraso mountain, with an altitude of 5124 m above sea level, still constitutes a prominent aspect of the topography of the area and is, in fact, the highest peak in the former Soras domain. Carahuaraso, Rasuwillka and Pumawanqa are the principal *wamanis*, i.e. mountains and mountain deities, in the Ayacucho area at present (Ansion, 1987: 138–9). Rasuwillka is located north of the town of Ayacucho. Another peak by the name of Rasuwillka, or Rasuhuilca is a mountain two km north-west of Carahuaraso, which is also over 5000 m in altitude, and is located in former Soras territory. The close proximity of these two peaks in the Soras territory suggest aspects of duality in them, and possibly paired deities.

The mountain bearing the name Rasuwillka, north of the town of Ayacucho, is also known to have been a major *huaca* during the Late Horizon (Guaman Poma, 1980: 248). The opposition of the Rasuwillka peak in Soras territory, to the Rasuwillka north of Ayacucho, in the opposite quarter or *suyu*, may be an indication of a religious landscape that stretched well beyond narrow ethnic boundaries.

It is worth noting that today the *wamanis* tend to be more closely associated with animal husbandry whereas the Pachamama is inclined to be more intimately connected with horticulture (Ansion, 1987), although of course this dichotomy is not to be seen as either absolute or indeed comprehensive.

In Albornoz's putting down of the Taqui Onqoy movement in the Soras area[5] he records the 32 *huacas* he and his collaborators destroyed in the village of Chicha (this site was

probably not Chiqna Jota itself but more likely the nearby Imglesia Chayoc[1]) as being *pacarinas* (see below), *llamayllas* (small personal amulets concerned with the fertility of llamas), *mamasaras* (small personal amulets concerned with the fertility of maize), and one *vilca* (idol), which were publicly burned (Millones, 1971: 4/23).

The *pacarinas* were defined by de Arriaga as 'which is where they say they descend from' (de Arriaga, 1968: 202). These places of origin without exception appear to have been springs, lakes, mountains or caves. *Pacarinas* appear to have constituted a point of contact in space and time between 'Uku Pacha' representing 'the world below or inside, past time and ancestors' and 'Kay Pacha' representing 'this world, present time and humans' (Anders, 1986: 911–6). These *pacarinas* are the places of origin from where humans, plants and animals emerged from Uku Pacha (Valcarcel, 1967: 153; Villena Aguirre, 1987: 176). They are therefore strongly connected with both fertility and the ancestors. Considering the nature of the context from which the offering of small and miniature pottery was recovered, a man-made structure, it seems unlikely that this constituted a *pacarina*.

ANCESTRAL BUILDINGS AND THEIR MEANING

Ancestral or ancient buildings, were frequently *huacas* in their own right. These structures did not represent temples for an object of veneration; they intrinsically constituted the object. In his description of the Inca Ceque system of the Cuzco area (a system of lines radiating out from a central point used to organize a social, political, temporal and ritual space), Bernabe Cobo cites numerous examples (1895: Vol. IV, 10, 13–14, 18–20, 23, 37 *passim*). They were typically houses in which Inca rulers were said to have lived or died, or which were associated with specific events in their lifetime. De Arriaga notes that in many parts of the highlands the houses of the 'Huaris' were worshipped. These were the first people of this land and they were supposed to have been giants (de Arriaga, 1968: 201–2). The term 'Huari' was associated with the people who traced themselves back to the land as opposed to the 'Ilacuaz' who were later settlers (Duvoils, 1986: 396; MacCormac, 1991: 413).

ETHNOGRAPHIC INFORMATION ON SMALL AND MINIATURE POTTERY

According to de Betanzos, in certain sacrificial burials, small vessels with *chicha* accompanied women who escorted a dead Inca (1987: 142). Garcilaso de la Vega refers to the use of a small cup in the Inti Raymi celebrations in Cuzco, in which the Inca would serve his kinsmen from a small cup by filling it from one of a pair of large beakers (probably *keros*). From one of these beakers he would drink himself and the contents of the other was served to the sun (Garcilaso de la Vega, 1723: 197). In these instances the references are to high-status events and the vessels were of precious metals. There is however no reason to believe that in lower status events similar vessels of less valuable materials would not be used.

The use of small vessels is also mentioned in connection with 'witches', as the Spaniards called native priests and shamans. These small pots were 'filled with concoctions of herbs, unctions and large live spiders, the vessels being sealed with clay' (Duvoils, 1977: 118). Spiders were used in soothsaying and augury (Rostworowski, 1988: 207).

Augustinian friars recorded the use of two small jars or pitchers in the Huamachuco area, called *magachi*, which were filled with water, and which were worshipped when the rains failed (Rostworowski, 1986: 81–2). Unfortunately it is impossible from the descriptions in most early texts to establish the exact definition of small when it is used as a qualifier.

Pottery was used both in the rituals associated with political and religious ceremonial, and on occasion it constituted the object of veneration or indeed a *huaca* in itself (de Arriaga, 1968: 230; Rostworowski, 1986: 81–2, 89–90). Albornoz specifically noted vases and cups decorated with figures as being used in ancient festivals and rituals in the Soras area (Duvoils, 1967: 22).

SOME ARCHAEOLOGICAL COMPARISONS

Small and miniature vessels, sometimes called toy vessels, are well known from the archaeological record. No exhaustive survey of all known examples is presented here. These types of pots occur certainly as early as the Early Intermediate Period as attested by examples from Pucara (Rowe and Brandel, 1969–70: 62–3, pl. XIV) and Qasawirka (Grossman, 1983: 98, fig. 53). They primarily occur in burials (Bennett, 1939: 59, 96, 102, 108; Menzel, 1966: 89), in caches in pits (Bennett, 1939: 67; Anders, 1990: 30–1) or ritually broken (Goldstein, 1993: 40). They are frequently coarsely executed (Bennet, 1939: 102) and there are both fired and unfired examples (ibid. 96, 108; Anders, 1990: 30–1). When they have been interpreted it has frequently been as childrens' toys (Bennett, 1939: *passim*). Some have indeed been found in the burials of children (ibid. 1939). Miniature vessels have been found associated with the bodies of children in a Late Horizon context at Tiwanaku (Ponce Sangines and Gregorio Cordero, 1969: 44), which have been interpreted as being associated with a Capac Hucha ceremony. The association of miniature and small pottery vessels with childrens' graves is not exclusively the case. They also occur in a Late Horizon context in association with female burials (Julien, 1990). It therefore seems that in the archaeological material the role of small and miniature pottery reflects a variety of uses which are very much context dependent but which are primarily ritual in nature.

INTERPRETATIONS AND CONCLUSIONS

The discrepancy between the two early documents referred to earlier, in the identification of the principal *wamani* of the Soras in the one text as Carahuaraso and in the other as Auqui Uscuntay, should be answered. It may well be explained by the presence of the three distinct language groups noted in the territory: Quechua, Aymara and Hahuasimi. If these reflect the existence of three separate groups which made up the Soras entity, then a different principal *huaca* or *wamani* for each would make sense. The identification of the main *wamani* would then be informant dependent, which would account for the confusion.

The tripartite division of the Soras area into Anansoras, Lurinsoras and Chalcos could also be a reflection of a multi-group make up of the Soras nation.

Chiqna Jota was a densely agglutinated settlement with evidence of organic growth. Clearly during the Late Horizon changes were forced onto this site as is evident from the

destruction of certain facilities and the building of structures with Inca-style characteristics. A number of routes of circulation within the site were blocked.

Some of the chronicle evidence suggests a violent conquest of the Soras area by the Incas resulting in major demographic adjustments (de Cieza de Leon, 1968: 129–32), although others contradict this sequence of events (Cobo, 1892: Vol. III, 162). There is indeed a reduction in the number of sites in the area from the preceding Late Intermediate Period of 21 to 8 (Meddens, 1985: 227–8). It should be noted though that the Late Horizon sites tend to be larger than the preceding Late Intermediate Period ones and that the time period involved for site development was significantly shorter. It seems fairly safe to postulate a continued role for a local élite at the site of Chiqna Jota in an intra-valley context. Indeed a maintenance and exploitation of the existing power structures by the Inca state appears likely.

To conclude therefore we can surmise from the historical sources and the archaeological record that the site of Chiqna Jota was occupied probably until early conquest times. It seems likely to have been abandoned before the reorganization of settlement structure under the Viceroy, Francisco de Toledo during the 1570's after the troubles of Taqui Onqoy.[5]

The archaeological, ethnohistorical and ethnographic sources provide indications of a variety of uses of miniature and small objects. These are most commonly of a ritual and religious nature. Small and miniature pottery is archaeologically known to occur in female and childrens' burials and in offerings. Ethnohistorically they are known to have been used in augery, in ceremonies, in offerings and as objects of veneration in themselves. The ethnographic sources indicate a link between miniature objects and preservation of well-being, fertility, ancestor worship and the *wamanis*. These types of objects are also known to be used as childrens' toys. By the nature of the evidence if these pots were childrens' toys, they would have incorporated another role when they became part of the offering context, as association with children may have produced in the vessels an inherent link with fertility.

The presence of Inca-style vessels and local Inca-style imitations in this offering should not be seen as rendering it a manifestation of an imperial Inca religious tradition. The inclusion of Inca-style material can be used as a chronological marker and an indication that there were no inherent contradictions between Inca religious practices and the local ones in this instance.

The offering appears to relate to a comparatively low stratum of the society, largely because of its poor quality. However the fact that it appears to have been a one-off, and was positioned in a prominent part of the site, makes it difficult to make status judgements.

Very tentative links with the worship of mountain deities may be implied by seeing the corridor as a point of contact with Uku Pacha. If this is the case, the corridor could have been viewed as an ancestral structure or as a building of the Huaris. These Huaris may have been similar to today's concept of 'Gentiles' (non-christian ancestors). As the structure formed part of the natural contour of the terrain, with a small entrance into the hillside, it might be consistent with the type of location where contact with Uku Pacha was possible. The structure might unite aspects of the *malquis*, in the sense of being a construction built by the ancestors or Huaris, with Uku Pacha because of its cave-like appearance, with a hole

connecting to the interior of the earth (Anders, 1986: 786, 788–91), providing a point of contact with the ancestors and the *wamanis*.

One other structure in the valley also suggests an association of a building with small window-like access points to a tradition of ancestor worship. This is a tomb construction inside a cave, dating to Middle Horizon 2. Entrance to this tomb was gained through small windows (Meddens, 1991: 228–9).

None of the sources describes exactly the use made of small and miniature pots as is the case here. Some general inferences and observations can be derived from the evidence however. The offering deposit in the gallery may be related to the cult of ancestor worship, in which the social and economic well-being, fertility or productivity of the community was ensured. This would follow if the structure itself was viewed as having been constructed by the ancestors or the *malquis* and in this context had been transformed into a *huaca* in its own right. This offering and the context in which it was made were undeniably linked to maintaining the social and economic well-being and fertility of the group that made it, whatever the specific religious details incorporated in the ritual that produced it.

In order to comprehend Andean religious ritual and beliefs, one has to realise that it was, and still is, inseparable from the rest of the Andean cultural experience. The political strategy of the Incas corresponded to the imperial necessity of maintaining the pre-existing economic and social order, as this formed the base of the precept of tribute (Ansion, 1987: 56), and was the one way in which the cycle of reciprocity between state, people and *huacas* could be maintained uninterrupted. In this regard the showing of respect to an ancestral structure in a non-Inca ethnic context during the Late Horizon can be understood. The use of the gallery as a *huaca*, explains the presence of the offering here.

Clearly the Late Horizon constituted a period of dynamic change and adaptation of this section of the Soras territory within the framework of the Inca state.

Abstract

An offering of small and miniature pottery vessels of Late Horizon date, placed in a gallery dating to Middle Horizon Epoch 2, was found at the site of Chiqna Jota in the Chicha/Soras valley, Ayacucho, Peru. Ethnographic and historical data suggests that the offering may be linked to ancestor worship to ensure enhanced productivity and fertility for the section of society responsible for the offering.

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Notes

1 During the reign of the Viceroy Francisco de Toledo, between 1571 and 1586, the local communities in the Soras area were relocated, ostensibly because their former abodes were the homes of infidels and idol worshippers (Varallanos, 1979: 39). Clearly the reasons behind this move had more to do with increasing the level of control over the resident population, both on grounds of security and taxation.

At least some of these new settlements were close to the original ones. With one exception the former are still occupied (Meddens, 1981: 209–10). The documents collected in *Las Informaciones de Christobal de Albornoz* (Millones, 1971: 4/18–27) referring to sites occupied before this enforced demographic shift, dealing with a campaign to wipe out idolatry by Christobal de Albornoz during the 1560s, have five or six settlement names in common with those named in a later document post-dating the enforced population movement, the *Relaciones Geograficas de Indias* (Jimenez de la Espada, 1881: Vol. 1, 171–2). These sites are Soras* (San Bartolome de Atunsora**; Anan Soras and Luren Sora***), Chilcayoc* (Santa Maria de Chilcayo**; Chillcayoc***), Matara* (Santa Maria de Matara**; Matara***), San Salvador de Quije* (San Salvador de Quiji**; Quisi***), (San Juan de Guaiguapata**; Guaigua***) and possibly Morcolla* (San Francisco de Morocolla**; Marquilla***).[6] All of these sites are or were located in the northern part of the Soras territory.

A village called Chicha, which is absent from the *Relaciones Geograficas de Indias*, is mentioned in the Albornoz documents. There now exists a hamlet of that name located near where the Rio Pachachaca joins the Rio Chicha. In the *visita* by Albornoz 32 idols from *caciques* (local rulers and community leaders) of the village of Chicha were burned and destroyed (Milliones, 1971: 4/23). Twelve natives from the village were punished for being involved with the Taqui Onqoy[5] movement, and as part of their punishment had to work in whatever capacity was required while the construction of the principal church of the village and of Atunsoras province went on (ibid. 4/18–19).

There are two important archaeological sites with evidence of significant Late Horizon components situated on the margins of the hamlet of Chicha. They are Chiqna Jota to the south and Imglesia Chayoc (meaning 'place of the little church') to the north-west of the village. At Imglesia Chayoc approximately eighty structures are present. The layout of the site is dispersed. Both circular and rectangular buildings occur, some with rounded corners and mostly arranged around patio-like open spaces. Some of the structures have benches made of fieldstone set in mud mortar around the interior walls. Both buildings made of roughly dressed fieldstone set in mud mortar, and polygonal ashlar constructions occur. The largest building is rectangular, gabled, and has two trapezoidal doorways in its north wall. This is the building locally identified as the *imglesia*. Both the long north and south walls have three niches and each gabled end has a circular window high up in the facades.

Pottery assemblages recovered from this site date to the Late Horizon and possibly the Early Colonial Period. The site is located on an important inter-valley route. Its original function may therefore have been that of an Inca *tambo* (station along a road, usually serving administrative and storage functions). The presence of what may be an Early Colonial church is suggestive of this being the 'Chicha' mentioned in the Albornoz documents. As the site of Chiqna Jota does not

appear to be mentioned in either the Albornoz documents or the *Relaciones Geograficas de Indias* (although it may of course be one of the unidentified ones in the former), it seems likely it had been abandoned by the time these *visitas* were made.

2 Outside measurements.

3 One small section of the gallery was left unexcavated. The roof was still in position here and it was decided, both for safety and preservation reasons, to leave this section intact. A small part of the offering was therefore left *in situ*. Additionally, not all the pottery from the relevant contexts was analysed. All special finds and a 10% stratified random sample of all diagnostic pottery was covered. This means that there is a chance that some of the broken or incomplete miniature and small vessels may not have been recognized and are therefore not included in the description here.

4 'Miniature' as used here refers exclusively to those vessel shapes which represent a small-size version of types more commonly found as full-sized vessels.

5 The expedition by Christobal de Albornoz against idolatry in the southern part of what is now the Department of Ayacucho was initiated by the discovery of a messianic movement called Taqui Onqoy during the 1560s.

6 * Sites on *Carta Nacional* 1:100,000, 1969, 28–o & 29–o.
** *Relaciones Geograficas de Indias* (Jimenez de la Espada, 1881).
*** *Las Informaciones de Christobal de Albornoz* (Milliones, 1971).

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Strategic considerations in dating, or 'How many dates do I need?'

by ALEX BAYLISS* and CLIVE ORTON**

INTRODUCTION

In the application of scientific dating techniques to provide archaeological dating evidence, there are many hurdles to be cleared before a set of determinations can be considered useful and reliable. They include:

(i) Is the technique accurate?
(ii) Is the sample contaminated?
(iii) Is the sample directly associated with the archaeological problem(s) we wish to solve?
(iv) Is the sample residual?
(v) What precision is likely to be achieved given the material available?
(vi) Is this precision adequate for the question(s) being asked of the determinations?

Of these, (i)–(v) have been the object of varying amounts of study and discussion (for a summary see Aitken, 1990: 76–107). Far less attention has been paid to (vi), and that usually in the form of cautionary tales (Campbell et al., 1979).

This hurdle is fundamental. If a set of determinations is unable in principle to answer the questions posed, the other hurdles become irrelevant. We need a way of assessing *in advance* whether a given set of determinations is likely to answer a given question. Alternatively, we can ask '*How many* determinations are needed to answer this question?' at a chosen level of statistical confidence.

Some implications for resource management arising from this approach are:

(i) if finance/material is not available to answer the question to an acceptable level of confidence, then there is no point in submitting samples that are insufficient or inadequate (in terms of their number, their precision, or their association with the archaeological problem),
(ii) we can avoid submitting too many samples,
(iii) we can reduce costs by lowering the confidence level required, but a point will come at which we have so little confidence in our answer that it is meaningless,
(iv) we can choose the dating technique that gives us the required precision.

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ARCHAEOLOGICAL QUESTIONS

It would be time-consuming to try to answer from scratch every such question of dating as it arises in archaeology. Fortunately we do not have to, as the majority of such questions are found to belong to a few basic types. So, if we can find answers to a few 'type-examples' of questions, we can extrapolate them to a wide range of practical questions.

In this paper, we shall attempt to solve archaeological questions about the use of radiocarbon dating, although the approach is equally applicable to any technique that yields probabilistic dates (e.g. from luminescence). It is not applicable to deterministic dates (e.g. from traditional dendrochronology). To be able to construct such a typology we first need a formal way of stating the problems. This is done by phrasing questions as statements with specific alternatives. For example, the question 'Is this event of Bronze Age date?' becomes 'This event is of Bronze Age date' with the alternative 'This event is of Iron Age date', since if we can successfully distinguish between Bronze Age and Iron Age events, we can also distinguish between Bronze Age and Roman, etc.

We have here introduced a key term, 'event', and implicitly another, 'phase'. An *event* is simply something that has happened at a point in time, or over a time interval so short that it appears as a point on an archaeological timescale (for example, the death of an organism, the manufacture of an artefact, the construction of an archaeological feature). By contrast, a *phase* is a span of time with a beginning and an end (which may or may not be known, but which are different on an archaeological timescale); it may correspond to an archaeological period, a calendrical date range, or a stratigraphic or ceramic phase.

The type of questions that will be examined here can be described as 'relative' or 'comparative', that is, they concern a comparison between two or more events and/or phases. The only exceptions will be questions concerned with the integrity of an event or phase.

It is crucial to realise that the apparently obvious question 'What is the date of this event?' is not really a question in its own right, but only a first step towards the explicit definition of one. The answer would be of no use in isolation; it becomes useful by being compared to the answers from similar questions about other events. So it must be followed by 'Is that contemporary with a certain event?', or 'Does that belong to a certain phase?', or perhaps 'How long is that before (or after) a certain event?' These comparisons can be made directly, without explicit reference to the actual dates. As so often in archaeology, a single piece of information is of little use; its value lies in the comparison with related pieces of information.

The first division in the typology is into three broad types:

Type 1: questions concerning the integrity of a single event or phase,

Type 2: questions concerning the relationship between two events and/or phases,

Type 3: questions concerning the relationship between three or more events and/or phases.

Each can be divided according to whether the question concerns events, phases or both. The types here are named from their type sites, in relation to which each question was first formally identified (see Case studies below):

Type 1a: questions concerning a single event (Irthlingborough type),

Type 1b: questions concerning a single phase (no type site yet),
Type 2a: questions concerning two events (1: Dover boat type, 2: Uxbridge type),
Type 2b: questions concerning two phases (1: Long Mynd type, 2: Yarnton type, 3: Salisbury Plain type),
Type 2c: questions concerning one event and one phase (1: Westhampnett type, 2: no type site yet).
Type 3 will not be considered here, although it has received considerable attention in the context of the Danebury excavations (Orton, 1983; Cunliffe and Orton, 1984; Naylor and Smith, 1988; Litton and Leese, 1991; Buck et al., 1992).

The types are described in more detail below:

Type 1a: the question is 'These determinations purport to relate to the same event. Are they really contemporary?' The statement is 'These determinations are contemporary', with the alternative 'These determinations are distributed throughout a phase of non-zero (but unknown) length.'

Type 1b: the question is 'These determinations purport to relate to a phase of a certain length. Do they?' The statement is 'These determinations relate to a phase of a certain length', with the alternative 'These determinations relate to a longer time period, which may or may not be a phase (i.e. it does not have to be unbroken).'

Type 2a: the question is 'Are the two events to which these determinations relate contemporary?' The statement is 'These determinations relate to two contemporary events', with the alternative 'These determinations relate to two non-contemporary events.' The distinction between the two sub-types is a statistical one (see below).

Type 2b: the first question is 'Are the two phases to which these determinations relate contemporary?' The statement is 'These determinations relate to two contemporary phases', with the alternative 'These determinations relate to two phases that are not strictly contemporary, but may overlap.' The second question is 'Are the two phases to which these determinations relate contiguous?' The statement is 'These determinations relate to two contiguous phases', with the alternative 'These determinations relate to two phases that are separated by a gap or lacuna.'

Type 2c: the question is 'Does the event to which these determinations relate occur within a certain phase?' The statement is either (2c1) 'The determinations relate to an event within this known phase' with the alternative 'The determinations relate to an event outside this phase', or (2c2) 'One set of determinations relate to an event which lies within the phase to which the others relate' with the alternative 'One set of determinations relate to an event which lies outside the phase to which the others relate.'

To make progress with these questions we must next formalise them.

STATISTICAL ANSWERS

For all types of questions, we adopt the statement as a formal null hypothesis (H_0) and the alternative as a formal alternate hypothesis (H_1). These will be in the form of statements about the relevant parameters of the questions, for example the true (but usually unknown)

dates of events and beginning and ending dates of phases. The data (the determinations) are represented by *test-statistics*, which are simple statistical summaries of them. Finally, we need a *rule*, which says that the null hypothesis is to be accepted if the test-statistic is less than a certain *critical value*, and rejected if it is greater than this value. The parameters and test-statistics for the various types are listed in Table 1.

TABLE 1. Parameters and test-statistics for types of problems.

Type	parameters	hypothesis		test-statistic
		null	alternate	
1a	$A, B, L = B - A$	$L = 0$	$L > 0$	$\mathrm{var}(x_i)$
1b	$A, B, L = B - A$	$L = L_0$	$L > L_0$	$\mathrm{var}(x_i)$
2a1	X, Y	$Y - X = 0$	$Y - X > 0$	$\bar{y} - \bar{x}$
2a2	X, Y	$Y - X = 0$	$Y - X \neq 0$	$\bar{y} - \bar{x}$
2b1	A_1, B_1, A_2, B_2	$A_1 = A_2, B_1 = B_2$	$A_2 \geq B_1$	$\bar{y} - \bar{x}$
2b2	A_1, B_1, A_2, B_2	$A_1 = A_2, B_1 = B_2$	$A_2 \geq B_1$ or $A_2 \geq B_1$	$\bar{y} - \bar{x}$
2b3	A_1, B_1, A_2, B_2	$A_2 = B_1$	$A_2 > B_1$	$\bar{y} - \bar{x}$
2c	A, B, X	$A \leq X \leq B$	$X < A$ or $X > B$	?

So far, we have a procedure for deciding whether to accept or reject a given null hypothesis in the light of a particular dataset (set of determinations). But this does not of itself tell us how many determinations we need to take. In an ideal world, we would like to be certain that we had made the right decision about the null hypothesis. Because we are using data that contain random errors, we can never achieve certainty—the best we can hope for is to be able to specify the probability of making the right decision. We can say 'Whether the null hypothesis is accepted or rejected, I want to be at least P% certain that this is the right decision.' In statistical language, this can be put as

prob(accept H_0 | H_0 true) $\geq$ P%, and

prob(reject H_0 | H_0 false) $\geq$ P%, at the same time:

this has been called Payne's criterion (S. Payne, personal communication, 1993). The first part is related to the conventional significance level of a hypothesis test (Fletcher and Lock, 1991: 60–2), but the second is less accessible. The problem is that while, if H_0 is true, there is only one possible value of the parameter and the probability of accepting H_0 can be calculated, whereas if H_0 is false, the parameter may take very many possible values, each of which will give a different probability of rejecting H_0. So how can we guarantee that this probability is at least the chosen level?

This brings us to the two remaining key concepts—*power functions* and *indifference zones*. The power function of a test is a graph that shows the probability of rejecting H_0 when the parameter takes various values (Fig. 1). The probability is least when the parameter takes

the value specified in the null hypothesis, and increases as the value of the parameter diverges from that value, tending towards, but usually not reaching, a value of 1 (or 100%). Use of this function exposes a problem with the criterion we have adopted, in that if the parameter takes a value very close to, but not the same as, the value in the null hypothesis, the probability of rejecting the null hypothesis (which is now false) is only slightly higher than it was when H_0 was true. We cannot achieve Payne's criterion if we stick to a rigid definition of 'false', i.e. if *any* value of the parameter that is not specified in H_0, however close it may be, is defined as 'false'. We overcome this problem by creating an indifference zone of k years either side (or, in some circumstances, on one side only) of the 'null' value of the parameter. In archaeological terms, we say that we do not mind if H_0 is false by a margin of up to k years, but if the difference is k years or more, we want Payne's criterion to hold. For example, in problem 2a1, imposing a zone of $\pm k$ years around the null value of zero

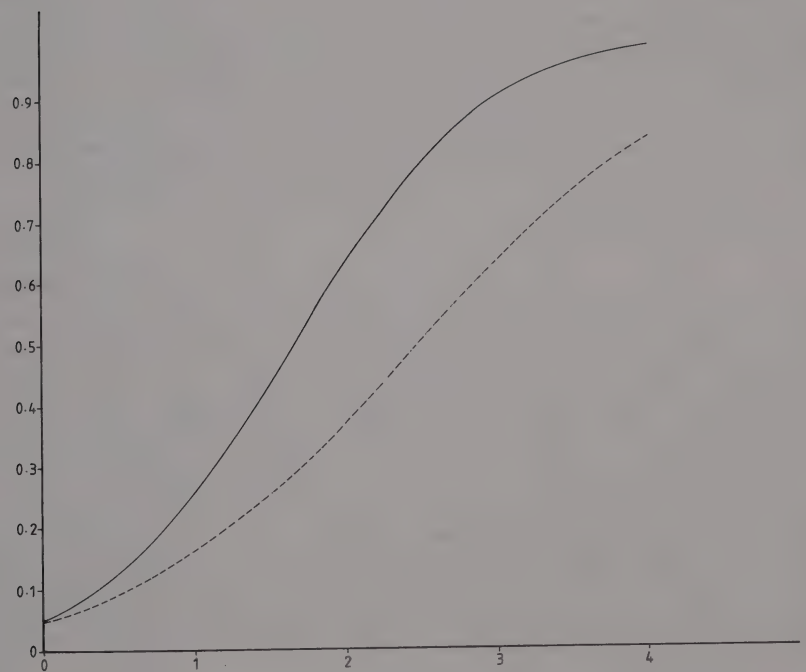

FIG. 1. Illustration of a power function (in this case, of a normal distribution). There is a null hypothesis H_0 that a certain parameter (e.g. the difference between the dates of two events) is zero. A statistical test tells us to reject H_0 if the difference between our estimate of the parameter and zero exceeds a certain critical value. The power is least when H_0 is true (i.e. when the parameter really is zero), and increases towards a value of 1 as the value of the parameter diverges from zero. A 'strong' test has a steep power function (solid line); a 'weak' test has a flatter power function (broken line).

would mean that we are prepared to accept events as contemporary provided that the true difference between their dates is less than k years. The choice of a value for k is an archaeological one; archaeologists who have been asked can with some help choose a sensible value; they seem to prefer to be asked to provide both an 'ideal' value and the largest value that would be of use to them.

The pieces are now in place for us to begin the modelling. We can express Payne's criterion as saying that the power when the parameter takes its null value (usually zero) is P% (or less), and that the power when the parameter is k years from this value is (100 – P)% (or more) (Fig. 2). Different sample sizes produce different power functions, but the power of a larger sample is greater than that of a smaller one, for all possible values of the parameter. The aim is to find the smallest sample size whose power function satisfies Payne's criterion; the formal solution will not usually be a whole number, in which case it must be rounded up to give the appropriate sample size.

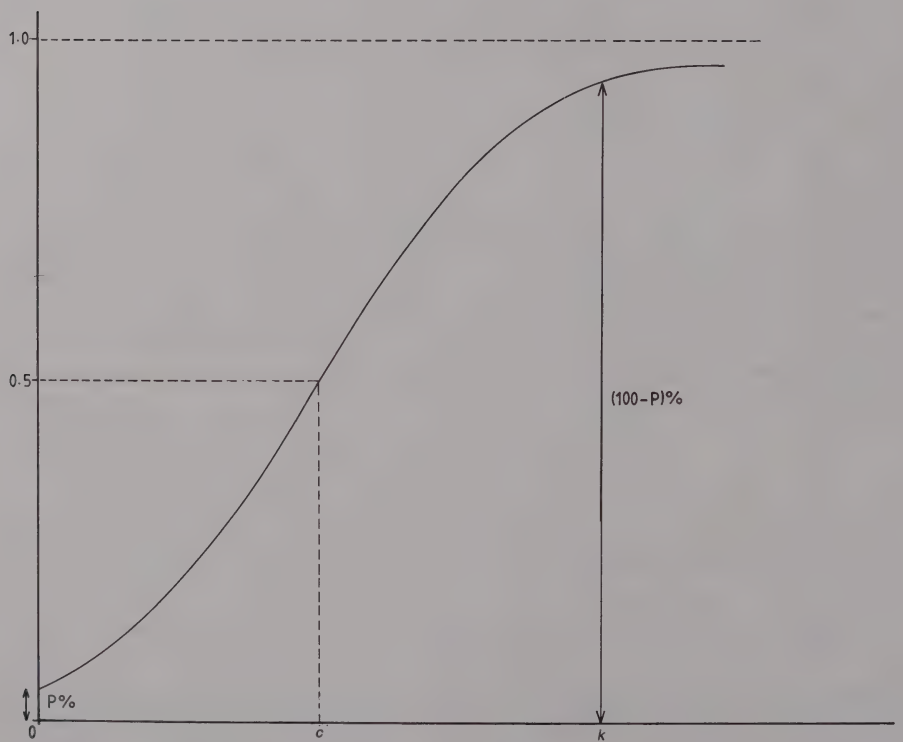

FIG. 2. The power function illustrated in Fig. 1, annotated to show the roles of the probability P, the size of the indifference zone k, the critical value c, and the way in which they are related through Payne's criterion.

We first calculate the critical value, c, that is needed to make Payne's criterion hold when the null hypothesis is true, i.e. we construct a rule to reject the null hypothesis if the value of the parameter is greater than c years. The critical value c can be expressed as a function of the error distribution of the determinations and of the sample size (Fletcher and Lock, 1991: 70–3). We next consider the situation when H_1 is true, and the parameter lies outside the indifference zone, taking the 'worst case' situation when it takes a value on the edge of the zone, i.e. the value k. In just the same way we can calculate a new critical value c' around k, such that H_1 will be rejected if the value of the parameter differs from k by more that c'. Just like c, c' is a function of the error distribution of the determinations and of the sample size. But since we must either accept H_0 or accept H_1 (there is no other choice), the critical value must be the same in both cases, i.e.

$$c = k - c'.$$

We have thus two equations for the value of c, each expressed as functions of the error distribution of the determinations and of the sample size. These can be equated, giving a new equation in which the only unknown is the sample size, n, which can be obtained by solving the equation. This process can be most easily followed in the case studies below. Note that in the case studies we are dealing with uncalibrated radiocarbon dates, which have a particularly simple error distribution (normal distributions with known standard deviations), so that an algebraic solution is possible. In real life, with calibrated dates, an algebraic solution is not possible, and a numerical solution must be sought (see Conclusions below). Nevertheless, uncalibrated dates give a useful demonstration of the method.

Case Studies

Irthlingborough (Type 1a)

The problem concerns an assemblage of nearly 200 cattle skulls from a Beaker period burial (Davis and Payne, 1993), and the question of the period over which the cattle were killed. At the extremes, one might envisage either a single gigantic barbeque or the steady accumulation of skulls and other bones over a period of hundreds of years. The archaeological question was phrased as 'Assuming a uniform rate of killing over the killing period...how many samples would one need to run to distinguish between a single killing episode and killing over (a) 1000 years, (b) 800 years, (c) 600 years, (d) 400 years, (e) 200 years, if the average 1 sigma error estimate on each determination is (w) ± 100 years, (x) ± 80 years, (y) ± 60 years, (z) ± 40 years?' (S. Payne, personal communication, 1993).

This problem can be restated statistically as follows:

There are n true dates X_i distributed uniformly over a period of L years (L = 0, 200, ... , 1000).

There are n related determinations x_i which are normally distributed with respective means X_i and common variance σ^2 (σ = 40, 60, 80, 100).

The null hypothesis is H_0: L = 0; there is a series of alternate hypotheses H_1: L = 200, ... , 1000.

The probability P = 0.95, i.e. 95%.

The test-statistic is s^2, the variance of the determinations $\{x_i\}$.

The rule is to accept H_0 if $s^2 < c$ and H_1 if $s^2 \geq c$, where c is the critical value to be determined (it depends on n, σ and L). The role of the size k of the indifference zone is taken here by the length L of the period. The algebraic calculations are given in Appendix 1. Table 2(a) shows the expected standard deviations of the determinations for various values of L and σ, while Table 2(b) and Figure 3 show the smallest suitable values of n for the same selected values of L and σ, with P = 0.95. For practical use the latter would have to be rounded up to the nearest even number.

On the data presented by Davis and Payne (1993, 16), i.e.

$n = 4$, s.d.$(x_i) \approx 100$, $\sigma \approx 80$,

Table 2(a) suggests that the best estimate of L is about 200 years, since a s.d. of 99 years is found in the 200 year column when $\sigma = 80$. However, Table 2(b) shows that we would need over 100 determinations to be '95% certain' that L is in fact 200 years.

There are four factors involved in these tables and their underlying equations: the s.d. (σ), length of period (L), number of determinations (n) and level of confidence (P). If we know any three of these, we can calculate the fourth. So, for example, we could ask 'What is the smallest date range that we could detect from a chosen number of determinations at a chosen level level of certainty?' (Table 2(b) shows it to be about 500 years if $n = 4$, $\sigma = 40$ years and P = 0.95).

TABLE 2(a). Expected values of the standard deviation of the determinations for selected values of L and σ for the Irthlingborough problem (Type 1a).

			L				
	200	300	400	500	600	800	1000
40	70	95	122	150	178	234	291
σ 60	83	105	130	156	183	239	295
80	99	118	140	165	191	244	300
100	115	132	153	176	200	252	306

TABLE 2(b). Required sample sizes for selected values of L and σ for the Irthlingborough problem (Type 1a) with P = 0.95.

			L				
	200	300	400	500	600	800	1000
40	19	9	7	5	5	3	3
σ 60	50	19	11	8	7	5	4
80	>100	35	19	13	9	7	5
100	>100	70	30	19	14	9	7

In more general terms, it is probably not practical except in special circumstances to detect a value of L of 200 years or less; for 400 years a σ of 60 years or less is needed, which seems to be well within the capabilities of present methods.

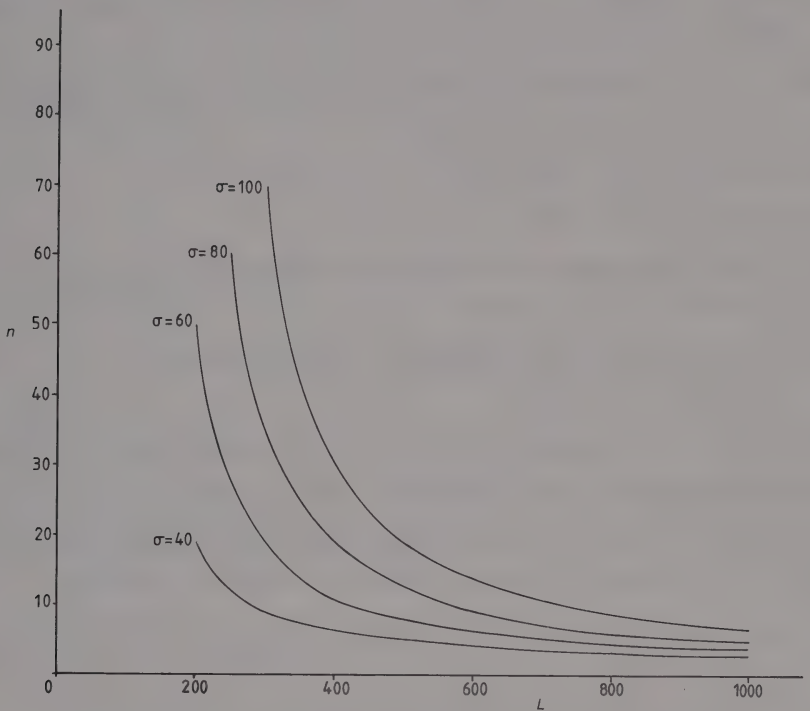

FIG. 3. Required sample sizes for selected values of L and σ for the Irthlingborough problem (Type 1a), with P = 0.95. See also Table 2(b).

The Long Mynd (Type 2b1)

Archaeological work on two earthworks on the Long Mynd, Shropshire, revealed a buried soil beneath each. Assemblages of charred plant remains have been recovered from both of the soils (C. de Rouffignac, personal communication, 1994). The excavator would like to demonstrate that the two soils are contemporary in order to compare the environmental assemblages. The archaeologist's 'best guesses' as to the dates of these features are:

1. Shooting Box Round Barrow—Bronze Age,
2. Devil's Mouth Cross Dyke—Bronze Age *or* Iron Age *or* middle Saxon.

The null hypothesis is that both the soils are Bronze Age; the alternative is that 1 is Bronze Age and that 2 is Iron Age (we ignore the possibility of it being middle Saxon, since

a sample size adequate to distinguish between Bronze Age and Iron Age will, *a fortiori*, be more than adequate to distinguish between Bronze Age and middle Saxon).

We have available n date determinations, which we can divide between n_1 samples from soil 1 and n_2 from soil 2, so that $n_1 + n_2 = n$.

We thus have the prospect of n_1 true dates $\{X_i\}$ distributed uniformly over a period of L_1 years from date A_1 to date B_1, and n_1 related determinations $\{x_i\}$ which are normally distributed with respective means X_i and common variance σ^2.

Similarly, we have n_2 true dates $\{Y_i\}$ distributed uniformly over a period of L_2 years from date A_2 to date B_2, and n_2 related determinations $\{y_i\}$ which are normally distributed with respective means Y_i and common variance σ^2.

The null hypothesis is H_0: $A_1 = A_2$ and $B_1 = B_2$ (so $L_1 = L_2$);

the alternative is H_1: $B_1 = A_2$ (and $L_1 \neq L_2$ necessarily).

The probability P = 0.95.

The test-statistic is the difference in the means of the determinations, i.e. $\bar{y} - \bar{x}$.

The rule is: accept H_0 if $\bar{y} - \bar{x} < c$; accept H_1 if $\bar{y} - \bar{x} \geq c$, where c is the critical value, to be determined.

The algebraic calculations given in Appendix 2 lead to the formula

$$n = 3.61 + 43.3(\sigma/L)^2.$$

The values of n implied by this formula are shown in Table 3 and Figure 4; for practical use they would have to be rounded up to the nearest even number. Table 3 shows that the number of determinations needed is higher the shorter the lengths of the phases, but the variation in this number is not great unless σ is high (e.g. 100 years). Low-precision determinations are almost as effective as high-precision ones for distinguishing between long phases, but high-precision ones have a distinct advantage for short phases.

TABLE 3. Required sample sizes (unrounded) for selected values of L and σ for the Long Mynd problem (Type 2b1), where P = 0.95.

	L						
	200	300	400	500	600	800	1000
40	5.3	4.4	4.0	3.9	3.8	3.7	3.7
σ 60	7.5	5.3	4.6	4.2	4.0	3.8	3.8
80	10.5	6.7	5.3	4.7	4.4	4.0	3.9
100	14.4	8.4	6.3	5.3	4.8	4.3	4.0

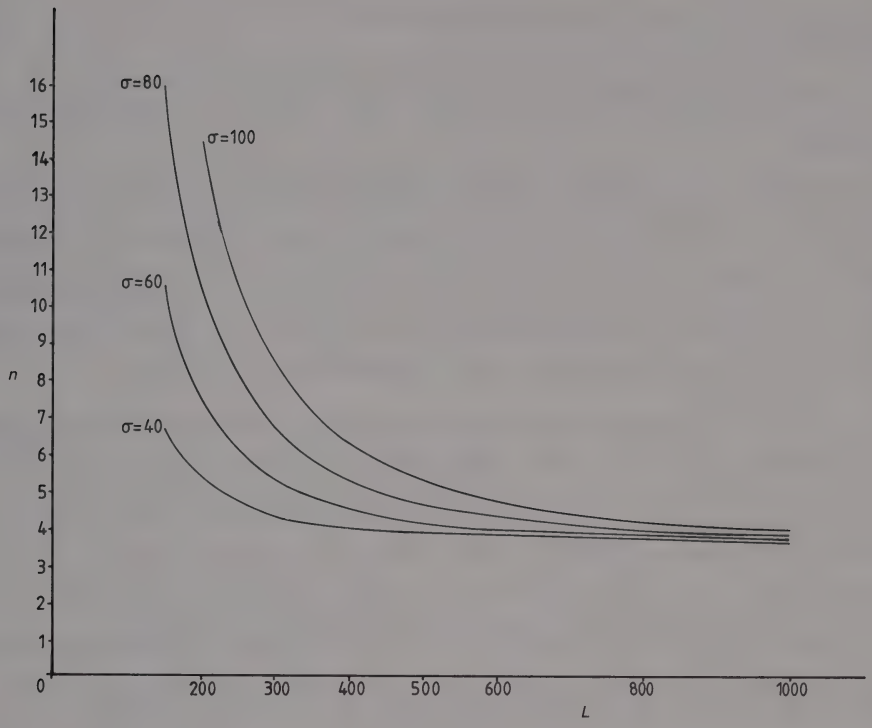

FIG. 4. Required sample sizes (unrounded) for selected values of L and σ for the Long Mynd problem (Type 2b1), where P = 0.95. See also Table 3.

The Dover boat (Type 2a1)

This question concerns the likely life-span of the Bronze Age boat recently excavated in Dover (Parfitt, 1993). The excavator would like to know how many determinations of what precision would be needed to distinguish between the date of the original construction (to be determined by dendrochronological analysis of the main structural timbers) and the date of the last repair of the boat (to be determined by radiocarbon determinations on the twisted yew withies which bound these timbers together). This problem depends on the wood technologist's view that the withies and the moss caulking would deteriorate rapidly and require replacement every few years—making the last repair contemporary with the abandonment on the radiocarbon timescale.

Statistically speaking, this is closely related to the Long Mynd problem above, the difference being that we are dealing with events instead of phases.
We have the null hypothesis H_0: $Y - X = 0$,

but the alternate hypothesis simplifies to H_1: $Y - X \geq k$.
The probability P = 0.95.
The test-statistic is the difference between the means of the determinations, i.e. $\bar{y} - \bar{x}$.
The rule is: accept H_0 if $\bar{y} - \bar{x} < c$; accept H_1 if $\bar{y} - \bar{x} \geq c$, where c is the critical value, to be determined.

This is again a one-tailed problem since the abandonment of the boat must be later than its construction: the corresponding two-tailed problem (Type 2a2) is from Three Ways Wharf, Uxbridge (Lewis, 1991). This is the first example in which we have had to explicitly introduce the size of the zone of indifference, k years; in the previous examples it was implicit in the formulation of the problem. The Dover boat is an atypical problem, since usually one would have to estimate the dates of both events. The algebraic calculations given in Appendix 3 are for the more usual situation and lead to the formula

$$n = 43.3(\sigma/k)^2.$$

This should agree with the formula for the Long Mynd type problem if we substitute 0 for L in that formula: a check shows that it does. When the date of one event is effectively 'known', as at Dover, the number of determinations needed is only 1/4 of the number given by this formula, contrary to the intuitive expectation that it should be 1/2 of the number.

The values of n that this implies for various archaeological choices of k and σ at Dover are shown in Table 4 and Figure 5, for practical use they would have to be rounded up to the nearest even number. Table 4 and Figure 5 show that for large zones of indifference (say 400 years or more), high-precision determinations have little advantage over low-precision ones, but as the length of the zone decreases the value of σ becomes more important. Indeed, for short zones (e.g. 50 or 100 years), high-precision determinations are essential.

TABLE 4. Required sample sizes (unrounded) for selected values of k and σ for the Dover boat problem (Type 2a1), P = 0.95. The symbol ... means less than 0.05.

	k									
	25	50	75	100	150	200	250	300	350	400
20	6.9	1.7	0.8	0.4	0.2	0.1	0.1	0.1	...	...
40	27.7	6.9	3.1	1.7	0.8	0.4	0.3	0.2	0.1	0.1
σ 60	62.4	15.6	6.9	3.9	1.7	1.0	0.6	0.4	0.3	0.2
80	110.8	27.7	12.3	6.9	3.1	1.7	1.1	0.8	0.6	0.4
100	173.2	43.3	19.2	10.8	4.8	2.7	1.7	1.2	0.9	0.7

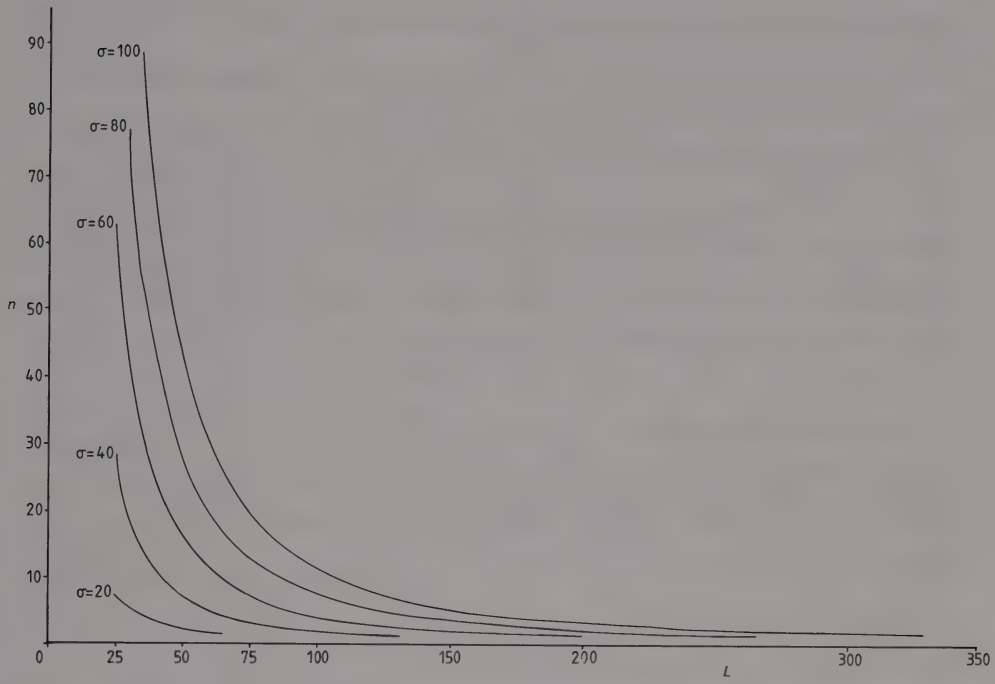

Figure 5. Required sample sizes (unrounded) for selected values of L and σ for the Dover boat problem (Type 2a1), P = 0.95. Compare Table 4.

CONCLUSIONS

The work undertaken so far has shown that questions asked by archaeologists of their dating evidence can, for the most part, be reduced to a relatively simple typology. For each type, a statistical theory can be developed to relate the precision of the determinations, the lengths of phases or of indifference zones, the required degree of certainty in an archaeological statement, and the number of determinations needed. These relationships can be used in several ways; given any three of the parameters listed here, we can use them to determine the fourth. Although we have concentrated here on asking 'How many determinations do I need?', one could equally well ask 'How confident can I be in making such a statement?' or 'How large must the indifference zone be if I am to be so confident of making such a statement based on so much evidence?'

Such questions are fundamental not only to archaeological interpretation, but also to archaeological planning and management. For example, the answer to the first question provides a rational basis for the allocation of resources to dating evidence. It can prevent waste that would arise from over-specifying the need for determinations, and can prompt

re-assessment of the question when the resources (in terms of money, material, or both) needed seem high in relation to its importance.

The second question is perhaps rather defensive, promoting a healthy caution and preventing embarrassment in the face of fresh evidence.

The third is possibly the most far-reaching, as it raises questions of what it is possible to say, even in principle. If, for example, the cost of reducing the zone of indifference between two events to less than 100 years is prohibitive (either in financial terms or the amount of material needed), then it will not be possible to distinguish archaeologically between the two events if they are separated by less than 100 years. It follows that it is not reasonable to build interpretations or theories that rely on the ability to do so. Some questions, therefore, are not worth asking because they can never be answered (at least, within existing technology). Either we must find a new technology to provide dating evidence, or we should not ask the particular question.

Much work remains to be done. We have here provided the theory for a few of the simpler problems; many others are to be studied, and above all, the implications of calibration of radiocarbon determinations need to be taken into account. This will involve numerical rather than algebraic, and specific rather than general, solutions, because a problem which can be answered in one period (where the calibration curve is well-behaved) may not be in another (where it is not). Nevertheless, we have here sketched the outlines of this work, and look forward to filling in the details.

Abstract

Some questions commonly asked of scientific dating evidence are classified into a typology. A general statistical theory is developed for the relationships between the precision of the determinations, the lengths of phases or of indifference zones, the required degree of certainty in an chronological statement, and the number of determinations needed. The theory is illustrated by three case studies based on current archaeological questions. Finally, the implications for archaeological interpretation are discussed and suggestions are made for further work.

Acknowledgements

The idea that scientific dating evidence should be amenable to this sort of treatment has been with us for some time. The initial impetus for getting down to work on it came from a request from Dr Sebastian Payne to look at the Irthlingborough problem. This was reinforced by the creation of an Institute of Archaeology Research Theme 'Managing and Conserving the Archaeological Resource', into which this work seemed to fit. The other problem types are based on applications for funding for scientific dating made to English Heritage. The figures were drawn by Neville Parker.

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Appendix I

We argue that if H_0 is true,

$\text{var}(x_i) = \sigma^2$ since all the X_i are the same,

but if H_1 is true, then

$\text{var}(x_i) = \sigma^2 + \text{var}(X_i)$.

If the X_i have a uniform distribution, then

$\text{var}(X_i) = L^2/12$ (Lindgren, 1960: 81), so

$\text{var}(x_i) = \sigma^2 + L^2/12$.

It follows that:

if H_0 is true, then

ns^2/σ^2 has a chi-squared distribution with $n - 1$ degrees of freedom,
so

$$c = (\sigma^2/n)X^2_{n-1}(0.95),$$

while if H_1 is true, then
$ns^2/(\sigma^2 + L^2/12)$ has a chi-squared distribution with $n - 1$ degrees of freedom, so

$$c = ((\sigma^2 + L^2/12)/n)X^2_{n-1}(0.05).$$

But c must be the same in both equations, so

$$(\sigma^2/n)X^2_{n-1}(0.95) = ((\sigma^2 + L^2/12)/n)X^2_{n-1}(0.05),$$

$$\text{i.e. } X^2_{n-1}(0.95)/X^2_{n-1}(0.05) = 1 + (L^2/12\sigma^2).$$

We solve this equation by plotting the value of the left-hand side for $n = 1, 2, \ldots$, and calculating the right-hand side for the specified values of L and σ.

Appendix 2

Now $\bar{x}$ has a normal distribution with

$$\text{mean } (A_1 + B_1)/2,$$

$$\text{variance } (\sigma^2 + L_1^2/12)/n_1,$$

and similarly $\bar{y}$ has a normal distribution with

$$\text{mean} = (A_2 + B_2)/2,$$

$$\text{variance} = (\sigma^2 + L_2^2/12)/n_2 \text{ (see above and Lindgren, 1960: 81).}$$

So the test-statistic $\bar{y} - \bar{x}$ has a normal distribution with

$$\text{mean} = (A_2 + B_2 - A_1 - B_1)/2,$$

$$\text{variance} = (\sigma^2 + L_1^2/12)/n_1 + (\sigma^2 + L_2^2/12)/n_2.$$

If H_0 is true, these simplify to mean = 0,

$$\text{variance} = (\sigma^2 + L^2/12)(1/n_1 + 1/n_2).$$

If L is known, we can use normal distribution theory. If L is not known but has to be estimated, then the use of normal distribution theory will slightly under-state the required sample size. We will use normal theory but keep this in mind.

Since we want prob(accept H_0 | H_0 is true) ≥ 0.95,
the critical value is given by

$$c = \sigma(\bar{y} - \bar{x})N(0.95) = 1.645\sigma(\bar{y} - \bar{x})$$

using the one-tailed probability since we have ruled out the possibility that $\underline{y} < \underline{x}$ on archaeological grounds. If this possibility had not been ruled out, we would have to use the two-tailed probability, which is the distinguishing feature of the Yarnton type of problem (Type 2b2).

Substituting for $\sigma(\bar{y} - \bar{x})$ leads to the equation

$$c = 1.645\sqrt{(\sigma^2 + L^2/12)(1/n_1 + 1/n_2)}.$$

By contrast, if H_1 is true, then the test-statistic $\bar{y} - \bar{x}$ has a normal distribution with

$$\text{mean} = (A_2 + B_2 - A_1 - B_1)/2,$$

$$\text{variance} = ((\sigma^2 + L_1^2/12)/n_1 + (\sigma^2 + L_2^2/12)/n_2).$$

Since we want prob(accept H_1 | H_1 is true) ≥ 0.95,
the critical value is given by

mean $- c = \sigma(\bar{y} - \bar{x})N(0.95) = 1.645\sigma(\bar{y} - \bar{x})$.

For purposes of illustration, we use the simple case when $L_1 = L_2$; the more general situation is still being studied. In the present case, the condition for H_0 gives

$c = \sigma(\bar{y} - \bar{x})N(0.95) = 1.645\sigma(\bar{y} - \bar{x})$,

while that for H_1 gives

$(A_2 + B_2 - A_1 - B_1)/2 - c = \sigma(\bar{y} - \bar{x})N(0.95) = c$.

Taking these two together we see that

$c = (A_2 + B_2 - A_1 - B_1)/4$,

but since, in our H_1, $B_1 = A_2$, we have that

$c = (B_2 - A_1)/4 = 2L/4 = L/2$.

We can now substitute for c in our equation and obtain

$L/2 = 1.645\sqrt{(\sigma^2 + L^2/12)(1/n_1 + 1/n_2)}$.

In the simple case when $L_1 = L_2$, the symmetry of the situation requires that $n_1 = n_2 = n/2$. So:

$L/2 = 1.645\sqrt{(\sigma^2 + L^2/12)(4/n)}$, so that

$L = 4 \times 1.645\sqrt{(\sigma^2 + L^2/12)/n}$, and

$\sqrt{n} = 6.58\sqrt{(\sigma^2 + L^2/12)/L}$, leading to

$n = (6.58)^2(\sigma^2 + L^2/12)/L^2 = 3.61 + 43.3(\sigma/L)^2$.

Appendix 3

If H_0 is true, then $\bar{y} - \bar{x}$ has a normal distribution with mean = 0,

variance $= \sigma^2(1/n_1 + 1/n_2) = \sigma^2(n_1 + n_2)/n_1n_2$.

The rule is to reject H_0 if $\bar{y} - \bar{x} \geq c$, i.e. if

$\text{phi}(c/\sigma\sqrt{((n_1 + n_2)/n_1n_2)}) \geq P$.

If H_1 is true, then in the worst-case situation the test-statistic $\bar{y} - \bar{x}$ has a normal distribution with

mean $= k$,

variance $= \sigma^2(1/n_1 + 1/n_2) = \sigma^2(n_1 + n_2)/n_1n_2$.

The rule is to reject H_1 if $\bar{y} - \bar{x} < c$, i.e.

$\text{phi}((c - k)/\sigma\sqrt{((n_1 + n_2)/n_1n_2)}) \leq 1 - P$.

From these two equations we can see that

$c/\sigma\sqrt{((n_1 + n_2)/n_1n_2)} = -(c - k)/\sigma\sqrt{((n_1 + n_2)/n_1n_2)}$, i.e.

$c = k/2$.

In this example, we have chosen P to be 0.95, so

$c/\sigma\sqrt{((n_1 + n_2)/n_1n_2)} \geq N(0.95) = 1.645$ and

$(k - c)/\sigma\sqrt{((n_1 + n_2)/n_1n_2)} \leq N(0.05) = -1.645$.

We argue from symmetry (see Appendix 2) that $n_1 = n_2 = n/2$, so that

$\sqrt{((n_1 + n_2)/n_1n_2)} = 2/\sqrt{n}$ and

$(k/2)/(2\sigma/\sqrt{n}) = 1.645$,

leading to $k\sqrt{n} = 4\sigma \times 1.645$,

i.e. $\sqrt{n} = 6.58(\sigma/k)$,

or $n = 43.3(\sigma/k)^2$

The Compton Bassett Area Research Project—first interim report

by ANDREW REYNOLDS*

with contributions by Paul Charlton, Alexander Deacon, Gabriel Pepper, Ellen Swift and Keith Wilkinson

The Compton Bassett Area Research Project, which was started in November 1991, is a multi-disciplinary study of settlement and land use in central North Wiltshire. The time span under study runs from the earliest evidence of human occupation in the area until the end of the post-medieval period.

This paper consists of an outline of the aims and objectives of the research programme, a series of preliminary reports on fieldwork carried out up to the end of 1993 and an assessment of current hypotheses about aspects of the development of the study area.

INTRODUCTION

The importance of landscape studies in archaeology has been recognized for a relatively short time. Large-scale survey and excavation programmes are of fundamental importance if we are ever to gain more than the over-simplified picture of patterns of settlement and land use which result from models constructed around individual sites, or groups of sites, assumed to be contemporary but which have been identified unsystematically. The number of variables involved in the evolution of a landscape is such that generalized models are nationally inapplicable.

A comparison of regional surveys indicates that highly complex and individual factors are involved in the development of particular landscapes, even though they may have common physical characteristics. For example, the cohesive rectilinear pattern of land division on the Dengie Peninsula, Essex is seen as Roman in origin (Drury and Rodwell, 1978: 137), and similar field systems in East Anglia (e.g. the Scole-Dickleburgh system) date probably to the Iron Age (Williamson, 1987: 429) whereas the earliest parallel reaves on Dartmoor are the product of land allotment and division during the Later Bronze Age (Fleming, 1978, 1983), the point being that landscapes of a similar character can be chronologically disparate.

The three examples quoted above illustrate continuity in the landscape. Studies elsewhere have indicated how settlement patterns are liable to both gradual and comprehensive change. Research into Anglo-Saxon and medieval settlement patterns in Norfolk has revealed a picture of fluidity in village morphology and location (Wade-Martins, 1980).

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Even though landscapes may be broadly categorized into two types, those of continuity and those of change, major alterations could affect the former type. This is to say that a region cohesively planned in the prehistoric or Roman periods will not necessarily remain fundamentally unaltered. The well-known case of the Wiltshire parishes of Alton Priors and Alton Barnes whose boundaries are clearly cut by Wansdyke shows how an organized system (in this case not of rectilinear type), although overlain by the dyke, still remained effective (Bonney, 1972). Perhaps, however, this has as much to say about the short-lived importance of Wansdyke as it does about the persistence of what became fossilized as parish boundaries.

There has been a tendency to overemphasize continuity in the landscape. Certainly there have been major swings in opinion from a viewpoint of continuity (e.g. Finberg, 1964a) and back again to one of change (Hooke, 1988: 3). It remains, however, that the English landscape is the result of a very lengthy process of evolution with its roots set in the Neolithic when organised territorial landscapes appear to have developed (see for example Drewett, 1977).

The level of determination exerted upon later patterns of settlement and land use by earlier systems can only be understood by systematic survey and excavation, as patterning suggested by, for example, SMR records and Ordnance Survey maps may be highly misleading (Reynolds, 1994). To summarize, the archaeological evidence points to overriding unpredictability in landscape archaeology. Detailed regional and local studies have been identified as the way forward by a number of leading fieldworkers (Aston, 1985: 152; Hooke, 1988: 8). The Compton Bassett Area Research Project has been initiated to contribute to the overall picture of landscape development on a national basis whilst providing a foundation for further regional and local research.

THE STUDY AREA

The study area is situated in central North Wiltshire (SU 07 SE; Fig. 1) and covers 24 square kilometres. It constitutes an area of diverse geology (Fig. 2), relief (Fig. 3), field systems (Fig. 4) and settlement morphology.

From west to east the geological strata consists of: Kimmeridge Clay, Lower Greensand, Gault Clay, Upper Greensand and Lower Chalk with the latter two deposits marking a rise in relief (from 105–175 m O.D.) onto the Lower Chalk shelf which skirts around the base of the Marlborough Downs. The soils vary accordingly, although post-depositional changes are apparent. These are twofold: hillwashing has caused quantities of both calcareous and acidic soils to move westward off and away from the downs and the accumulation of colluvium in dry valleys along the scarp slope has affected their morphology.

Three named waterways run through the study area: the Abberd Brook to the north-west of Compton Bassett, River's Brook to the north of Cherhill and the Winterbourne to the north and east of Yatesbury.

The field systems on the chalk downland are markedly different in form to those on the sands and clays. Those on the chalk appear to have been laid out between a series of long boundaries which run both concentrically around the base of the Marlborough Downs and

FIG. 1. Location of the study area in Southern England.

FIG. 2. The geology of the study area. From west to east: Kimmeridge Clay, Lower Greensand, Gault Clay, Upper Greensand, Lower Chalk.

west/east from the edge of the scarp slope. The fields to the west of the scarp slope show all the signs of a more piecemeal, or aggregated, development, although episodes of formal planning are apparent.

There is very little woodland in the area, with what little there is being situated on the sands and clays. The chalk downland is under a predominantly agrarian regime whereas the sands and clays support a more mixed farming economy.

There is notable climatic variation either side of the scarp slope. Yatesbury is colder and wetter than Compton Bassett. Accordingly there is a difference of two weeks in harvesting between the two localities.

The three villages in the study area are all very different in plan (Fig. 5). In a strictly morphological sense Compton Bassett is a dispersed settlement of the 'interrupted row' type (Dyer, 1990; Reynolds, 1993) with houses set both sides of a single road which follows the bottom of the scarp slope. Yatesbury is a shrunken settlement. The present village lies some 300 m to the east of the church and consists mainly of one row of houses along the eastern side of the most easterly of two parallel roads. Cherhill is a nucleated village with a more formal plan consisting of houses situated between three parallel roads.

METHODOLOGY

Coding

All fields in the study area are coded according to the farm to which they belong. The first part of the code is alphabetical and is determined by the initials of the farm. The second part is numerical and refers to a running sequence of numbers given to fields. For example, the fields belonging to Yatesbury, Manor Farm are titled YMF 1–26. Excavated trenches are coded alphabetically and context numbers numerically. All artefacts are marked with the full code which, for Yatesbury, Manor Farm, may read YMF 1, A, (226). Sites on small holdings or in gardens are given initials relating most clearly to their location. Church surveys are coded using the initials of the village and the dedication.

Field survey

A programme of individual field recording is underway. This involves producing a standardized description of aspects of each field using a pro forma record sheet. Details recorded include composition of boundaries, present and previous (if known) land use, soil type(s), topography and, depending on land use, a brief description of any upstanding earthworks, artefact scatters or other significant features. Earthworks are graded with a simple two-part code. The first part is alphabetical and describes clarity: A = prominent, B = visible and C = ephemeral. The second part is numerical and interpretative: 1 = settlement, 2 = field system and 3 = other (recorder to elaborate). Recommendations are made by the recorder if more detailed survey is considered necessary.

Fieldwalking

Fields are divided into 10 m squares and walked intensively with the aim of recovering the

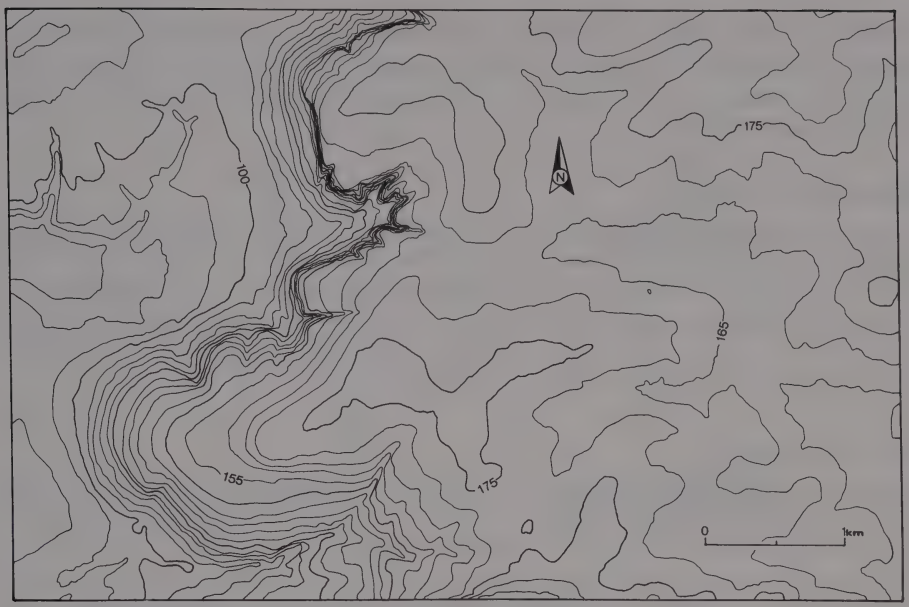

FIG. 3. Contour map of the study area.

FIG. 4. The field systems and sites and monuments of the study area. Filled circles = round barrows, open circles = ring ditches, squares = findspots, thick lines = Wiltshire County Council SMR's, medium lines = existing boundaries, thin lines = SMR's identified by CBARP.

maximum amount of material. Squares are numbered in a running sequence for each field. Material of all periods is collected as patterns of artefact dispersal in relation to existing settlement foci are to be used to test the validity of assumptions about site location suggested by surface collection data.

Retrogressive analysis

The study of stratigraphic relationships of boundaries and roads has the potential to reveal a detailed relative sequence for the division of the landscape. The process involves the removal of features shown on the earliest reliable maps in order to reveal earlier patterns of land division and/or primary boundaries. The technique has been adequately described elsewhere (Williamson, 1987, 1988). Aerial photographs provide much useful evidence and all relationships are ultimately checked in the field.

Environmental survey

Augering and test pitting of valley bottom sediments is in progress and will continue until a sufficient quantity of data is recovered. In addition, sediment and monolith samples are taken when appropriate. Sieving and flotation is carried out accordingly.

The contrast of calcareous and acidic soils east and west of the scarp slope allows for the analysis of molluscan data from downland sites to be compared with pollen evidence from the sands and clays.

Documentary research

The documentary record is to be analysed in three stages. Firstly, the location, nature and content of individual sources is to be established. In order of importance, documents are to be described and transcribed into a single typescript. Following this, the record is to be interpreted.

Place-name analysis

Place-names and field names are studied to provide information about previous land use and allotment.

Standing buildings

The study of standing buildings is considered in two parts: analysis of the structure and a consideration of the location of the building in relation to its surroundings. Structural survey consists of producing composite and phased ground plans and the recording of significant elevations at a scale of 1:10 or 1:20 depending on the level of detail required. Distinct elements are described using pro forma record sheets. Mouldings and other architectural details are drawn at a scale of 1:1.

Excavation

Small-scale excavation is carried out with the aim of dating field boundaries and other

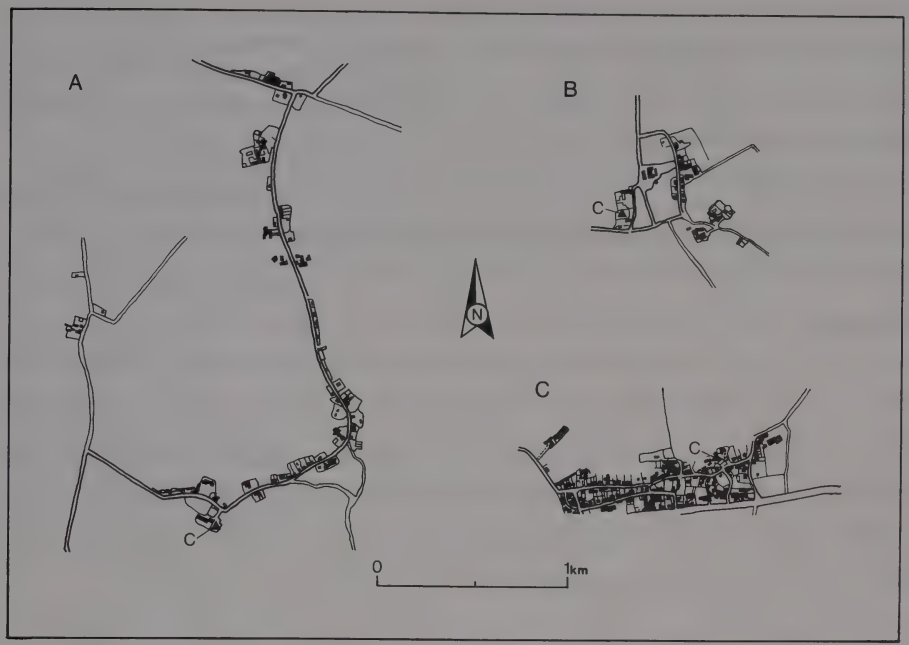

FIG. 5. Comparative plans of (A) Compton Bassett, (B) Yatesbury and (C) Cherhill. c = church.

similar features identified from aerial photographs. Test pitting is undertaken at locations where fieldwalking suggests concentrated activity or where the limit or nature of known deposits is to be established. Larger scale excavations are carried out when it is felt that the data recovered would be of significant value. Three-dimensional plotting of artefacts is widely used in order to extract the maximum amount of information from data recovered. The site recording system consists of two parts: the written record and the drawn record. The former comprises a series of pro forma record sheets which are designed to be both user friendly and comprehensive. All descriptions are standardized and the recording system is fully cross-referenced. The drawn record is compiled using a system of single-context planning (cf. Spence, 1990, section 2) in conjunction with the recording of sections and profiles. All features are recorded in plan at a scale of 1:20 and in section at 1:10. When necessary more complex features are drawn at 1:5 or 1:1.

THE ARCHAEOLOGY OF THE STUDY AREA: AIMS AND OBJECTIVES

The chalk downland in the Avebury area has long been a focus for archaeological research. Fieldwork has, in the main, been limited across the landscape west of the causewayed enclosure at Windmill Hill. It is a major aim, therefore, to explore the relationship between downland and vale over time.

Ceramic traditions in North Wiltshire from the later prehistoric through to the early post-medieval period are poorly understood and it is hoped that the excavation of stratified sites will eventually provide a dated type series. Coupled with the results of fieldwalking and environmental survey, it should then be possible to assign broad dates for periods of clearance and arable farming.

The archaeology of the study area is presented by traditional period division, although it should be stressed that the evolution of the landscape is a continuous process. The details below are not a definitive statement of the present state of research and only sites and finds of significance are considered, so as to provide a general overview.

Mesolithic

The earliest evidence for human activity in the area comes from Cherhill (SU 0311 7005) where, in 1967, excavations by John Evans and Isobel Smith revealed a remarkable sequence of deposits dating from the Late-glacial to the Late Neolithic/Early Bronze Age (Evans and Smith, 1983). A Mesolithic occupation horizon with an associated lithic industry and faunal assemblage was a particularly important discovery. Flintwork has been recovered through fieldwalking at Freeth Farm, Compton Bassett and from the excavations at Manor Farm, Yatesbury. Casual finds including an axe, or adze, and a micro blade core have been made at Four Acre Covert, Compton Bassett.

Neolithic

The Neolithic archaeology of the Avebury area is well known and constitutes listing as a World Heritage Site by the United Nations. The study area is situated adjacent to this landscape, although relationships between the downland and the sands and clays has not been explored. Preliminary fieldwork and chance finds have indicated the potential of the study area in understanding exploitation of the landscape to the west of the chalk.

Six polished axes as well as leaf-shaped and transverse arrowheads have been found within a 500 m radius at Freeth Farm (centred on SU 0374 7120) at the western end of the study area (Charlton, forthcoming). At Four Acre Covert (SU 039 710) two discoidal scrapers have been found. All of these artefacts were casual finds and a programme of systematic fieldwalking is regarded as a priority in these localities. There is a possible cursus at Yatesbury, although this has yet to be confirmed (Whittle, 1993: 36). A flaked axe and a fragment of a polished axe have been recovered from the excavations at Manor Farm, Yatesbury, where an east–west oval mound is currently being investigated. Information from fieldwork relating to this period can be used to complement the results of the large-scale research programme currently being undertaken by University of Wales, College of Cardiff (ibid.).

Bronze Age

The Earlier Bronze Age is represented primarily by a linear cemetery of five round barrows, on a north-west/south-east axis, at Yatesbury (Fig. 4). A number of these were investigated

by Dean Merewether and Arthur Charles Smith in the mid nineteenth century (A.C. Smith, 1884). The two most southwesterly barrows are suffering severe plough damage and it is hoped to gain access for the purposes of survey. A group of three ring ditches, visible as cropmarks, lie 1.2 km to the north of Yatesbury (centered on SU 060 729). There is a ditchless conical mound at Mount Wood, Compton Bassett (SU 0324 7311) which may date to this period. A number of finds have been made in the vicinity of Cherhill including a biconical urn (I.F. Smith, 1961).

The Later Bronze Age archaeology of the Marlborough Downs has been studied in detail (Gingell, 1992) which provides a backdrop for research into this period. Evidence has so far been almost exclusively limited to a number of casually collected sherds from around Yatesbury, although a particular concentration has been noted 500 m south of the village (SU 0666 6662) (R. Lucas, personal communication, 1993).

Iron Age

Finds of early Iron Age date have been made at Four Acre Covert, Compton Bassett (SU 039 710). These include fragments of furrowed bowls and a triangular loomweight. These finds almost certainly represent a settlement site on the west-facing scarp of the downs. The site may be a farmstead possibly associated with the hillfort at Oldbury, 2.5 km to the south-east.

Over a dozen quernstone fragments have been found around Freeth Farm, Compton Bassett, some of which may date to this period. It is planned to conduct a fieldwalking survey of the Four Acre Covert site followed by test pitting to determine the nature and extent of any archaeological deposits.

Roman

Evidence for this period is represented by a general scatter of pottery, brick and tile in the ploughsoil over much of the study area, and as residual material in most of the excavation work so far undertaken. There is evidence for a pottery kiln at The Manor Farm, Compton Bassett (SU 0330 7297; Frere 1987: 345). It is intended to carry out a magnetometry survey and excavate the site, with the aims of locating any ancillary structures and producing a dated type series for the most locally produced fabric. Three-dimensional plotting may enable the identification of domestic and work areas through the presence of 'alien' sherds (cf. Freke and Craddock, 1980: 15). It is also hoped that the excavation of stratified deposits will add to the existing body of evidence for the dating of other regional pottery types. More tangible settlement evidence is so far restricted to the site at Cherhill (Johnson and Walters, 1988) where buildings and a mosaic were uncovered. However, a number of features of Roman date have been sampled through excavation.

Anglo-Saxon

Study of the Anglo-Saxon period in North Wiltshire has been focused largely on the development of towns, particularly the tenth-century burh at Cricklade (Haslam, 1976, 1981, 1984). The Charter evidence has been fairly well explored, although only one spurious

example survives which may relate to the study area. A charter exists which, it has been suggested, may refer to Compton Bassett (Finberg, 1964b: no. 212). The general consensus, however, is that this is not the case (Sawyer, 1968).

In the early part of this century G.B. Grundy undertook a detailed study of roads drawing largely on charter bounds for his information (Grundy, 1918). Since this date one short paper on Anglo-Saxon roads has examined some aspects of the evidence for communications (Pelteret, 1985). The subject of roads is a much neglected area of research. There has been no consideration, for example, of the development of *herepath* (army way) or *portway* (market way) routes (Hill, 1988) or any attempt, for that matter, to distinguish between the two systems. Particular emphasis is placed on the study of roads and boundaries in the study area. Significant results have been obtained with regard to Anglo-Saxon and medieval routes (see below: Deacon, 1994).

Secondary early Anglo-Saxon inhumations in round barrows are common in Wiltshire. At West Overton five secondary interments in barrows of Roman and Beaker date provide the earliest ceramics for comparison and are dated to the sixth century by association (Eagles, 1986). In 1833, a number of inhumations were discovered during the levelling of a barrow in Yatesbury field (A.C. Smith, 1884: 87). A date in the seventh century is suggested by the metal 'workbox' found with one of the skeletons. Such finds are frequently associated with cemeteries of 'Final Phase' type (Meaney and Hawkes, 1970; Boddington, 1990).

Settlement patterns are a neglected area of research, although early Anglo-Saxon settlements have been identified at Swindon, Avebury and Liddington Hill (Eagles, 1986: 110, n. 7; Fowler and Walters, 1981: site 25). Accordingly the quantity of artefactual material is limited. Domestic pottery in the county is scarce, especially before *c.* 850. The recently published ceramic sequence from Trowbridge Castle runs from *c.* 850 and provides the nearest stratified material for comparative purposes (Graham and Davies, 1993).

The location of early and middle Anglo-Saxon settlement foci is of prime importance. A quernstone and pottery sherd of probable middle Anglo-Saxon date have been found at Freeth Farm (SU 0200 7256). No Anglo-Saxon material has been forthcoming from Cherhill. At Manor Farm, Yatesbury notable quantities of ceramics have been recovered from stratified contexts (see below).

The origin and development of churches is an important area of research. There may be some Anglo-Saxon fabric at St Swithun's Church, Compton Bassett (Reynolds, 1993: 106) but no diagnostic features have been recognized. The intimate relationship between the church and Manor House at Cherhill is of particular interest. The two structures, both of roughly similar proportions, are set end to end on an east-west axis with the church to the east of the manor. Although close proximity of a church and manor is commonplace, the arrangement at Cherhill is unusual and bears a striking resemblance to the situation of structures on certain high-status early and middle Anglo-Saxon sites. It would appear that the original churchyards at all three villages were once much larger before becoming reduced by as much as three-quarters to their present sizes. Exploratory excavations are currently underway adjacent to All Saints Church, Yatesbury on an area formerly within and on the bounds of the early churchyard.

Medieval

Study of the development of the landscape in the medieval period is aided by the survival of both documents and place\field names. Medieval ceramics are difficult to date or provenance, particularly the coarse wares. Although both Nash Hill and Minety products are present (McCarthy, 1974; Musty, 1973), there are other fabrics where the point of origin is not known. The lack of well-stratified urban sites in the area is very marked and has hampered the dating of rural sites.

The development of road networks throughout the period is being explored with a particular emphasis on distinguishing droveways from other routes and the modification of earlier roads.

There are upstanding earthworks around all three villages and at other locations in the study area there are field systems including lynchets and traces of ridge and furrow. Great importance is placed on understanding the nature of economic exploitation during the medieval period and in attempting to identify changes in patterns of land use. Progress has been made in this respect through both survey work and excavation (see preliminary reports for Compton Hill and Roach Wood below). Preliminary fieldwork has indicated the specialized functions of the scarp slope.

The origin and development of the three villages is clearly dissimilar. Survey work has been particularly successful at Yatesbury and Compton Bassett where aspects of village development have been elucidated (see below).

The study of churches is largely, although not exclusively, concerned with the medieval fabric. As the survey of churches in the study area has now been completed the database is being enlarged by additional surveys of churches adjacent to it. Avebury, St James (Semple, 1994) has been subject to the first of these 'extra-mural' surveys. The long-term aim is to study churches in a regional context. Architectural mouldings are studied with a view to isolating local characteristics.

The recent recommendations on research priorities made by The Society for Medieval Archaeology (Hinton, 1983) have been taken into account in the aims and objectives of the Project's medieval studies.

Post-Medieval

Changes to the landscape in this period have involved episodes of wholesale re-planning and re-organisation which have created the appearance of much of the area as it is today. How, when and why these changes came about is an important area of research. Post-medieval structural phases of churches are examined in detail.

EXCAVATION AND FIELDWORK 1991–3: A PRELIMINARY REPORT

Fieldwork to date has involved all of the techniques outlined above. The following summary reports describe the results of all investigations up to April 1993 (Fig. 6). Sites are discussed in relation to research problems specific to the study area. Comparative discussion will be undertaken in the final reports.

Excavations on a flight of scarp-face strip-lynchets at Roach Wood SU 041 782 (Fig. 6.1)

The scarp-face strip-lynchets at Roach Wood are situated between the 125 m and 155 m contour lines. They are the most prominent of a series of south-facing strip lynchets that are a feature of the coombes to the north-east of the village of Compton Bassett. They consist of six terraces, (L1) – (L6), which run from about a quarter of the way down to the bottom of the scarp slope. Following an evaluation, in December 1991, a 10-day excavation programme was designed to answer specific questions:

i) What was the sequence and relationship between the lynchets and their enclosing ditch system?
ii) At what date was the system formed/cultivated/abandoned?
iii) Did the lynchets serve a horticultural or agricultural purpose and what crops were grown?

The excavations

Five trenches A–E were located in an attempt to answer these questions (Fig. 7). These cuttings are described in chronological order of the features investigated. All trenches at Roach Wood were 1 m wide. In all cases each of the longest sections were drawn as the opposing sections in a trench were often different. As a result, each section was issued with its own set of context numbers. The positions of finds, where possible, were plotted onto one section drawing for each trench.

The lynchets and their enclosure were divided into four phases:

Phase 1

The uppermost lynchet (L1) is much denuded, is of lesser proportion and has a smaller area than those below it. Trench D, which measured 11 m x 1 m, was located near its eastern end. On excavation the section (Fig. 8a) showed a fully mature soil profile with clear A and B horizons. The negative part of this lynchet was cut into hillwash material (87). The probable cultivation soil (26) was between 20 and 30 cm deep and consisted of a fine sandy clay moderately interspersed with fragments of Upper Greensand. Beneath this a thin layer of weathered parent rock material (27) directly overlaid Upper Greensand, the natural bedrock at the site. The positive accumulation was insubstantial and it appears as though a level platform for cultivation was never realized. Particle-size analysis of sediments and soils from the hillwash material (87), (96) and (97) and from the positive lynchet accumulation (23), (24), (25), (26), (27) and (28) is to be carried out to assess the nature of their deposition. Molluscan analysis is to be carried out to establish whether species introduced during the Roman period occur within the cultivation soil or the hillwash deposit. No dating evidence was recovered.

Phase 2

The lynchets below (L1) are more prominent with sharp profiles and flat surfaces. On excavation, lynchet (L2) was shown to be more complex than (L1) (Fig. 8c). Trench C, which measured 9 m x 1 m, was located near its eastern end. Below a slightly less well-

FIG. 6. Location of sites investigated. 1 Roach Wood, 2 Starve Knoll, 3 South-west of The Manor Farm, 4 West of New Covert, 5 Pleasure Ground, Freeth Farm, 6 Chapel Coombe, 7 Compton Hill, 8 St Swithun's Church, 9 Yatesbury, Manor Farm, 10 All Saints Church.

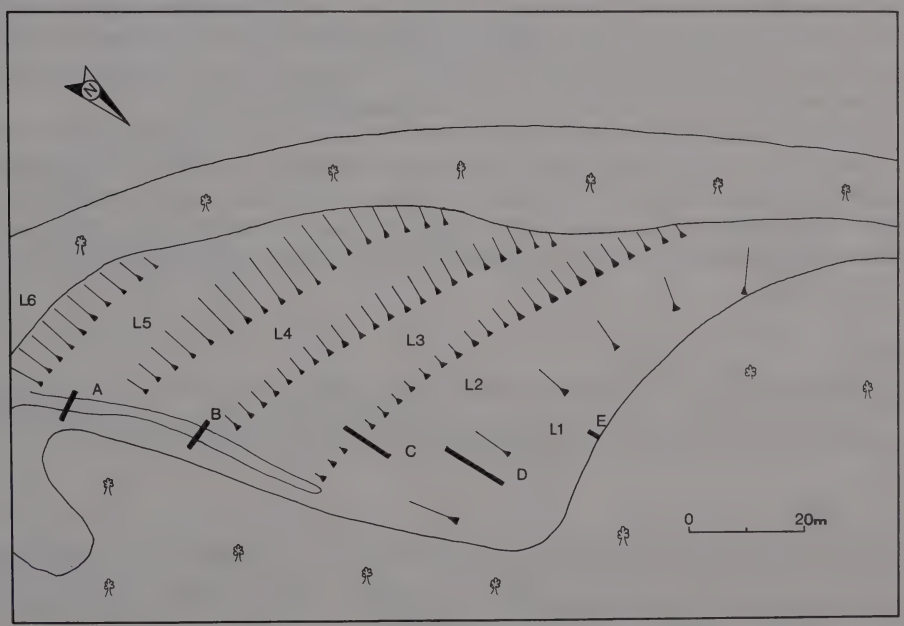

FIG. 7. Preliminary plan of scarp-face strip-lynchets at Roach Wood.

developed soil profile was a possible cultivation soil (114) which was between 30 and 40 cm deep. Below this was a layer of chalk fragments (94) which is probably best interpreted as marling of the acidic Upper Greensand derived soils. Below this a layer of fine sandy clay (95) overlaid the natural bedrock and a lens of silty clay (115) which sealed a shallow pit-like feature (90) (Fig. 8d) with a fill of chalk fragments within a sandy clay matrix (89). This feature may be a natural hollow filled with hillwash derived from the lower chalk, in which case the layer above it may represent the remnant of a pre-lynchet soil. Particle-size analysis is to be applied to this problem. Finds from (L2) (Fig. 8b) included a single sherd of much abraded Romano-British pottery recovered from the possible pre-lynchet soil which provides a *terminus post quem* for the formation of the lynchet. Pottery of the same period was also recovered from the upper levels of the lynchet.

Phase 3

The system is enclosed on its northern and eastern sides by ditches which, in places, have a slight bank. The ditch on the eastern side was trenched in two places, half-way down the slope and at the bottom of the slope.

Trench B, which measured 6 m x 1 m, was positioned at the eastern end of (L4) where the bank, which was situated to the east of the ditch, appeared most intact. As a steep coombe bounds the system on its eastern side it seems most likely that for its greater part the bank has become eroded away through slippage. The ditch (13) clearly post-dates the lynchets, as it was cut through a probable cultivation soil (9) and (7), which appeared on both sides of the ditch cut and what appeared to be a partly developed soil (4) (Fig. 8e). This suggests that the ditch system was cut some time after cultivation had ceased. Additionally the tail end of the negative part of (L4) could be seen continuing on the eastern side of the ditch (Fig. 8f). A jetton dated to 1550–85 was recovered from the bank context (5).

Trench A measured 6 m x 1 m and was located further down the slope across the ditch at the eastern end of (L5). It was hoped that a trench positioned at the bottom of the slope would be well placed to recover material discarded in the ditch which had washed down the slope. Unfortunately no finds were forthcoming from the ditch fills. The bank at this point had been badly disturbed by roots and generally the stratigraphy was less informative than that in trench B.

Phase 4

Trench E measured 3 m x 1 m and was located across the ditch (to the north of the lynchets), which appears to be the latest feature at the site. On excavation it was shown to be insubstantial. Its fill consisted almost exclusively of humic material.

Discussion

The system appears to have started with the formation of (L1). Its eroded state and different character to the lynchets further down the hill suggest a date earlier than the main flight, possibly prehistoric or Roman, although without dating evidence such conclusions will have to be tested against the results of molluscan analysis.

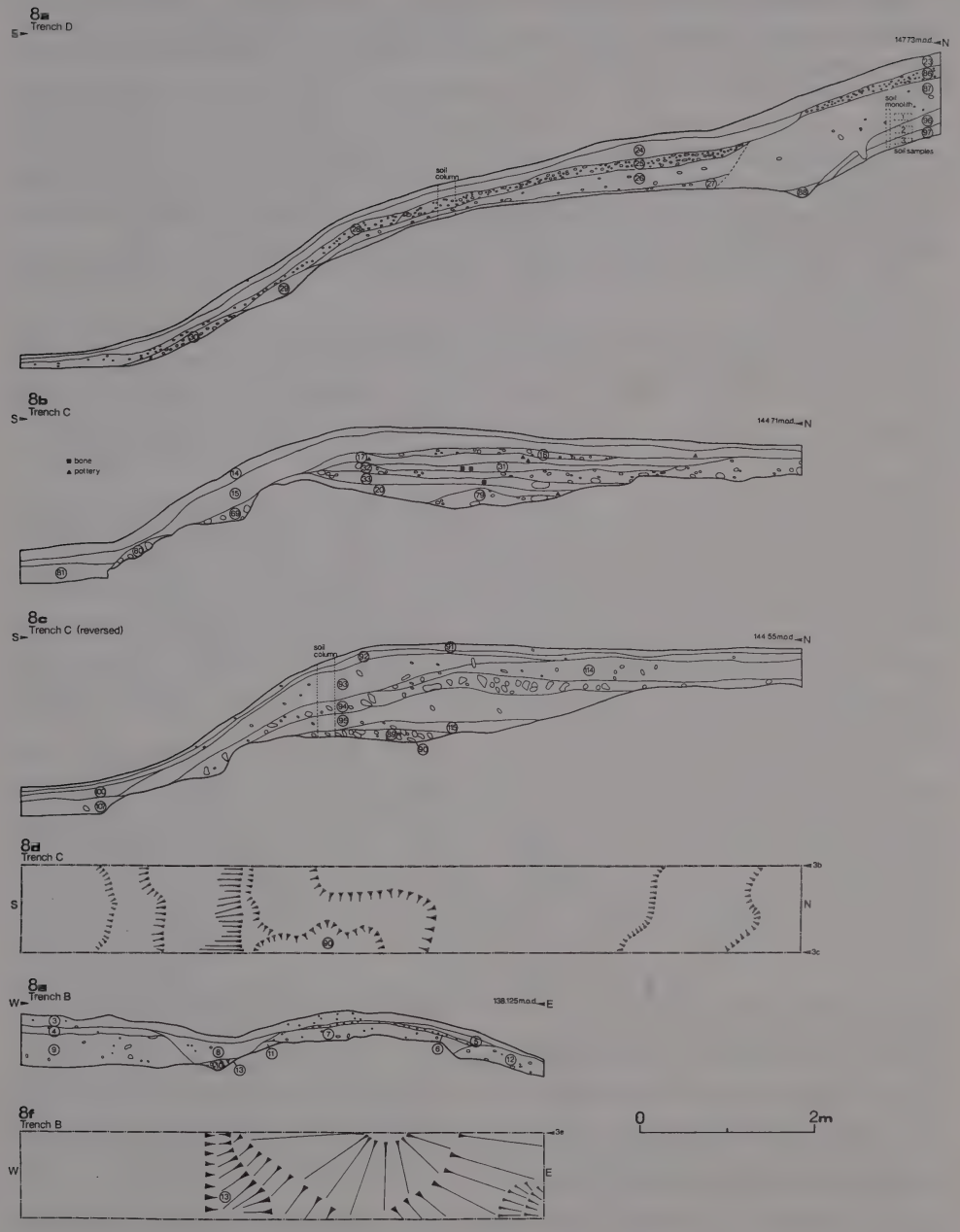

FIG. 8. Roach Wood sections and plans.

The main flight of lynchets are still well defined and the jetton contained within the material forming the enclosing bank suggests that cultivation had ceased by the earlier part of the sixteenth century as a soil had begun to form over the cultivation level before the ditch was cut. The ditch on the eastern side only extends up the hill to the riser of (L2) which supports the hypothesis that (L1) is chronologically unrelated to the rest of the system. It appears that after cultivation was no longer practised the site was enclosed for the purpose of keeping animals (probably sheep) on the slopes. A ditch would have been necessary on the eastern side to prevent animals from falling into the steep-sided coombe and it seems likely that the limit of the enclosure to the north was the positive accumulation of (L2).

The ditch to the north is presumed to be recent due to its ephemeral appearance and shallow depth.

Identifying the types of plants grown on the lynchets is a complex problem. Although residues of the plants themselves are highly unlikely to be preserved in an acidic environment, chemical analysis of soils should give some indication of the type of fertilizer employed. This then makes it possible to at least infer groups of plants that were likely to have been grown using a certain type of fertilizer. Manuring from domestic middens is unlikely as finds were so few.

The proportion and area of the lynchets raises questions about workability by plough teams. The available turning area at either end of any of the lynchets is not great and the way in which they drop sharply at their northern termini, to meet with the unmodified ground surface, suggests access onto the platforms would have been difficult to achieve.

Apart from agricultural usage there is the possibility of a horticultural function. The size of the lynchets is easily explained in terms of their formation over a long period as the result of ploughing action, but if they served a horticultural purpose then they must have been, to some extent, deliberately constructed.

The reason most frequently cited for lynchet formation in the medieval period is the process of bringing marginal land under cultivation during periods of intense pressure on the land during the twelfth and thirteenth centuries. If the possible scale of cultivation on the strip-lynchets at Roach Wood is viewed in context with the production capacity of the surrounding landscape it is clear that the return from the cultivation of arable crops is unlikely to have had much impact on a hungry population. This factor should be viewed alongside a consideration of the length of time involved before a worthwhile return of any type of crop can be obtained from a flight of lynchets, since they do not form over a short period of time if they are purely the result of ploughing action. The system at Roach Wood may have had its origins in the twelfth and thirteenth centuries as a response to land pressure due to population increase but, for the reasons outlined above, this is considered unlikely. Climatic deterioration throughout the fourteenth century may have been the motivating force behind their formation, as much of the agricultural land around Compton Bassett is situated on poorly draining sands and clays. If such land became unworkable an option would be the cultivation of the scarp slope.

To summarize, the deliberate construction of the lynchets for some form of specialist horticulture is thought most likely, especially as the field system is south facing. As certain

crops require, or are suited to, particular conditions it would not be exceptional to modify a suitable location for the growing of cash crops. Any further discussion will have to await the results of the scientific analyses.

Excavations at Starve Knoll SU 0452 7277 (Fig. 6.2)

A section was cut through the bank and ditch that runs north-east/south-west across Starve Knoll. It was hoped that a substantial ditch would be found thus providing a sequence of deposits suitable for the study of environmental change. The knoll itself has been disturbed to a considerable extent by chalk pits. Excavation clearly showed that the bank had been constructed from upcast from the chalk pits and that an insubstantial ditch some 40 cm deep had been cut through the same debris. The bank was revetted on its western side with a low wall constructed from chalk blocks and consisted of simple dumps of material with compacted lower levels. The turf was poorly rooted to the bank and the top levels were loose. The ditch was filled with re-deposited upcast from the chalk pits and topsoil.

Discussion

Although the bank and ditch seemed before excavation to be later than the chalk pits, it was considered that the division of such a small heavily disturbed area with such a substantial boundary was unusual and the possibility existed that the feature represented an early land division that had been respected until recently. The majority of the chalk pits on the knoll are probably post-medieval/early modern and the construction of a substantial boundary in the form of a bank is not common in these periods. Although no dating evidence was recovered from the bank, ditch or upcast, the chalk pits can probably be related to the extraction of Lower Chalk for the construction of houses in the locality during the eighteenth and nineteeth centuries. The hollow ways around both sides of Starve Knoll are almost certainly connected with the transportation of material from the top of the knoll to Compton Bassett. Considering the intensity of extraction at the site it seems as though the Lower Chalk here is of a particularly high quality and it may well be that quarrying here has a medieval origin. The thirteenth-century north aisle of All Saints Church at Yatesbury, 2 km to the west, is partially constructed of Lower Chalk. This indicates that the material was extracted from an early date in the region and that it was deemed suitable for use in high-status buildings.

Excavations south-west of The Manor Farm SU 033 729 (Fig. 6.3)

A 1 m x 1 m test pit was excavated to confirm and assess deposits of Romano-British pottery, charcoal and burnt clay recognized in 1986 during the digging of a pipe trench (Frere, 1987: 345). Below approximately 40 cm of ploughsoil a layer of orange and red burnt clay was recognized, flecks of charcoal were few but present and a number of sherds of Romano-British pottery were recovered including several base sherds. The trench was not continued further as it was only intended to expose the archaeology for the present time therefore leaving deposits intact.

Discussion

It is suggested that the area examined is on the periphery of a kiln site (M.A. Loft, personal communication). The resources necessary for production are all close at hand. There is an outcrop of workable clay visible in the stream course 75 m to the south-east of the site. A magnetometer survey is proposed.

Excavations west of New Covert SU 022 730 (Fig. 6.4)

A trench measuring 50 m x 1 m was dug westward from the edge of New Covert along the northern side of a field boundary which directly overlies SMR No. SU07SW612, described as a rectangular enclosure. The trench was dug down to the depth of the natural deposit of Lower Greensand. No features were encountered. A further trench 15 m x 0.5 m was dug at 90 degrees to the first trench in an attempt to locate the northern side of the enclosure; again no features were located.

Excavations at the Pleasure Ground, Freeth Farm SU 0238 7260 (Fig. 6.5)

Two trenches, A measuring 3 m x 1 m and B 6 m x 1 m, were located to examine features described as stone walls, which had been found by Mrs S. Henly when digging holes for tree planting. Directly below the turf a surface consisting of mortared coarse rubble was encountered in both trenches. In Trench A the northern limit of this feature was established and consisted of the remnant of a rubble wall. Trench B was extended to see if any limit could be established to the south. An edge was found and the east-facing section of the trench clearly showed the mortared surface lying over a layer of re-deposited sand. This in turn overlaid a lighter sand above the natural geology of Lower Greensand.

Discussion

In plan the trenches appear to have been located in the corner of a courtyard or building with a mortared floor. The site itself was in a cultivated field until the immediate post-war period and was fenced off from the cultivated area due to the position of trees which hampered ploughing (R. Henly, personal communication). The condition of the mortared surface suggests that the area examined was probably not ploughed. Further work is to be carried out to obtain dating evidence from beneath the floor.

Environmental survey at Chapel Coombe (by Keith Wilkinson) (Fig. 6.6)

Chapel Coombe is a dry valley of approximately 70 m in length by 8 m in width (measurements of the valley floor). It is situated on the south-east side of the larger dry valley stretching from the village of Compton Bassett to Starve Knoll and has been cut into the chalk escarpment, probably by spring sapping during the last glacial (Devensian). In an attempt to map Holocene sediments eroding from the chalk escarpment to the north, and from these to reconstruct past environments and erosional history, a series of bore holes and a test pit were excavated during November–December 1991 and April 1992. The main focus of this study was on colluvial sediments produced since the Neolithic by agriculture on the

chalk. These deposits have been shown to contain a variety of evidence as to past land use and environment (Bell, 1983).

Auger Survey

A total of 11 bore holes were drilled laterally along the valley at either 10 or 15 m intervals. The equipment used was a screw auger with a total of 6 m of extension tubing (although in practice only 4–5 m of this was ever needed). All observations were logged using Munsell colour charts and a standard description technique (Avery, 1980). A screw auger cannot accurately record stratigraphy due to inherent alterations to sediment compaction and distribution by the use of this method. However, it was the only type of auger able to penetrate the common stony layers found at Chapel Coombe.

Notes on sediment types (only notes are presented here, full sediment descriptions will appear at a later date)

The modern soil A horizon is typical of those found covering colluvial deposits on chalk. It is extremely humic and of an almost uniform depth of 18–20 cm. The B horizon is much less humic and has much material incorporated into it from the colluvial layers below, probably brought up by the actions of earthworms.

The colluvial layers vary a great deal with both depth and lateral distance. For this reason it has proved difficult to map them accurately over long distances. Basically there can be seen to be two types of deposits, one coarse and one fine.

The coarse deposit, as the name suggests, is made of a great deal of large–medium chalk which appears to have eroded into the valley during storm events leading to rill and gulley erosion of the valley sides. The finer deposit is made up largely of silts and clay, and has obviously accumulated gradually over a much longer period of time. Common to both deposits is the presence of iron staining which suggests deposition under oxidising conditions possibly produced by alternate wet and dry periods.

In two bore holes a deposit was found underlying the colluvium which could represent a paleosol. This has characteristics common to the modern soil (i.e. similar crumb structure and inclusions of similar quantities and sizes of chalk pellets), but it is not humic.

Laboratory analysis

A screw auger does not give a good indication of precise sedimentary relationships, nor is it a reliable way of taking samples. A series of samples were, however, taken from bore holes 1 and 3 for preliminary investigation. Analysis of these showed that lower levels of the colluvium contained loess, suggesting its presence in former chalkland soils. If this was the case the character of the soils must have been very different to those of the present day. In layers above the loessic sediments quantities of mollusc shell were found including *Vertigo pygmaea*, *Vallonia costata*, *Vitrea crystallina* and *Punctum pymaeum*. Although only a few in number, these suggest grassland conditions at the time of deposition. However, the very presence of mollusc shell shows promise for future environmental studies on these deposits. No mollusc shell was found in the paleosol despite a pH of 8.

The test pit

This was excavated with the intention of confirming the auger data in the northern part of the valley. It was also intended that dating evidence would be recovered to provenance the various colluvial episodes. The test pit measured 2 m x 1 m.

Colluvial sediments only extended to a depth of 1.65 m. Underlying them was the chalk bedrock. This result is somewhat surprising as auger holes both to the north and south showed colluvial depths of around 2 m. A possible explanation is that there is a hollow to the north of the trench into which a greater depth of colluvium has eroded. The possibility that the auger holes were not bored at exactly 90 degrees to the ground surface cannot be ignored, but if this were the case it would mean that all auger holes were bored at a non-90 degree angle.

Unfortunately no pottery was found to date the colluvium and the three bones found were all identified as sheep/goat.

Conclusions

The above data shows that large quantities of colluvium have accumulated in Chapel Coombe, suggesting large-scale agriculture on the chalk escarpment in the past. Today this erosion is continuing but seems to be caused largely by rabbit action on the actual slopes of the valley. Modern agriculture is still intensive but seems to have solved the problem of soil loss downslope through the construction of field boundaries well away from the lip of the dry valley. Obviously this was not the case in the past.

Bore hole 11 produced some very peculiar results in the lack of any sediments between 2.0 and 3.2 m. Locals have suggested this could be the route of a subterranean stream, but the location seems a little too high for this.

The presence of loess in the colluvial sediments is particularly interesting as it probably dates the soil that was the source of this colluvium to the Bronze Age or earlier. This suggests that colluviation has been taking place since at least this time.

Field survey at Compton Hill (Fig. 6.7)

Field survey revealed traces of ridge and furrow preserved within the width of a system of green lanes. The recognized remains constitute a rare survival on the chalk downland and indicate large-scale re-organisation of the landscape associated with the enclosure of open fields.

Village development

Significant progress has been made in understanding the development of medieval settlement (Reynolds, 1994). The morphology of each village is briefly discussed.

Compton Bassett

Compton Bassett now has a dispersed plan but preliminary study has indicated that this is probably a post-medieval phenomenon. A series of aerial photographs, taken by the RAF

shortly after World War II, indicate that the village was formerly nucleated and surrounded by open-field systems. Tenements and other boundaries have been distinguished, and traces of ridge and furrow are clearly overlain by later field boundaries. The main concentration of settlement appears to have been some 250 m north-east of the church. The existence of some form of settlement by the late Anglo-Saxon period is revealed by three entries in the Domesday Survey for Wiltshire (Thorn and Thorn, 1979: 27-2, 32-3 and 67-3) which probably correspond to three separate manors. The location of these three manors has yet to be fully established. Compton Farm and the former Compton Bassett House are likely contenders but positive identification will have to await the results of documentary research.

St Swithun's Church, Compton Bassett (Fig. 6.8)

Since St Swithun's Church has been the subject of a detailed survey (Reynolds, 1993), only a brief description of the results will be given here. There are seven major structural phases visible in the surviving fabric, beginning with a two-celled masonry church in the late eleventh or early twelfth century, culminating in the modern church, heavily restored in the late nineteenth century. The building is notable for its late twelfth-century capitals and fine fifteenth-century rood screen which may originate from Salisbury Cathedral (ibid. 110).

Cherhill

Cherhill does not appear in the Domesday survey, although this does not preclude early origins for the settlement as it may have been part of the Royal Manor of Calne (Blackford, 1941: 38). Compared to the other settlements the known archaeology of Cherhill is more impressive. The church and manor overlie a fourth-century Roman building with mosaic floors which have been paralleled with others of Christian character (Johnson and Walters, 1988). The relationship of the church and manor (see above) may suggest that these structures overlie buildings of early or middle Anglo-Saxon date.

The early focus of the village appears to be around the church where the road system and property boundaries are less rigid. The settlement appears to have undergone a westward shift along, possibly, formally planned streets.

Yatesbury

Manor Farm, Yatesbury (with Alexander Deacon and Ellen Swift) (Fig. 6.9)

Yatesbury is mentioned in the Domesday Survey (Thorn and Thorn, 1979), although earlier origins have been suggested by the results of excavations at Manor Farm (Fig. 9) where the earliest feature consists of part of a probably circular enclosure dated to the late Roman period. On first inspection Yatesbury appears to be a shrunken nucleated settlement. Present occupation lies some 300 m to the east of All Saints Church and consists mainly of one row of houses along the eastern side of the most easterly of three parallel roads. Initially the village was seen as having shifted in a northerly and easterly direction from an early focus around the church. Field survey has indicated that the village was quite different in the Anglo-Saxon, medieval and post-medieval periods, having a more dispersed plan form.

Three enclosures strung in a line from west to east have been recognized. A cutting through the most easterly of these produced evidence of Roman activity, with subsequent evidence of maintenance in the early and middle Anglo-Saxon periods and on up to *c.* 1750 (Fig. 10). There is evidence for small-scale iron working in the later medieval period and other finds worthy of some note include two writing leads of medieval date. Yatesbury seems to have developed its dispersed plan form, retaining certain elements from its foundation in the late Roman period. Comprehensive re-planning has occurred in the post-medieval period giving us the present plan form (Reynolds, 1994).

All Saints Church, Yatesbury (with Paul Charlton) (Fig. 6.10)

Seven major structural phases were recognized dating to between the twelfth and nineteenth centuries. The first phase appears to have consisted of a nave and chancel. In the late twelfth or early thirteenth century a south aisle was added and a north aisle followed probably in the middle of the thirteenth century. In the later fourteenth or fifteenth century the south aisle either collapsed or was demolished leaving two piers visible inside the nave. Further alterations and additions were carried out in rapid but distinct succession throughout the fifteenth century. A tower was added at the west end followed by a clerestory. Subsequent to the re-fenestration of the south wall of the nave, a stair turret, serving a rood loft, was butted against the south wall and both aisles were embattled. The chancel was replaced in the nineteenth century.

The Avebury–Yatesbury Herepath*; an Anglo-Saxon system of communications and civil defence (with Gabriel Pepper)*

Studies of civil defence in the later Anglo-Saxon period have concentrated on the fortified sites listed in the *Burghal Hidage* of 911–14, a full transcript of which remained unpublished until 1969 (Hill, 1969). Research has sought to identify secondary defensive centres in the landscape and associated contemporary road networks. Excavation and fieldwork at Avebury and Yatesbury (see above), have suggested the existence of a *herepath* (army way) route running from Marlborough to Broad Hinton, and possibly as far as Wroughton. Work to date has been concentrated on this small part of a much more comprehensive network of roads evident in the northern part of the county in particular. The course of the *herepath* considered here follows the broad shelf of lower chalk which skirts the Middle and Upper Chalk of the Marlborough Downs. The route has been identified using evidence from topographic and place-name analysis. A study of the stratigraphic relationships between the *herepath*, field boundaries and other roads has indicated its primacy as a determining feature in the landscape.

Place-name evidence

The *herepath* is considered below from the point where it departs from Marlborough where research by Jeremy Haslam has indicated a late Saxon stronghold, centred on St Mary's church (Haslam, 1976: 41). The route passes northward through Marlborough along Kingsbury Street, a significant name in itself, which, Haslam suggests, indicates the position of a royal

FIG. 9. Plan of earthworks at Yatesbury, Manor Farm showing position of trench B (Fig. 10).

FIG. 10. Trench B section Yatesbury, Manor Farm.

residence (*villa regalis*). Kingsbury Street is the most southwesterly of three parallel roads which demark the area of the tenth-century *burh* and runs north-west out of Marlborough up onto Manton Down, across Fyfield Down, and descends into Avebury, entering the henge monument through its east entrance. The section immediately to the east of Avebury is still known as a *herepath*. The route leaves Avebury through its western entrance, crosses the Kennet by way of a ford, and runs in a northwesterly direction to Yatesbury, where the results of recent work have suggested that the route originally ran through the most easterly of three enclosures. North of Yatesbury the route can be postulated as far as Broad Hinton, and perhaps as far as Wroughton, where 'Beran byrg' is mentioned in *c.* 890, meaning 'Bera's burh' (Gover et al., 1939: 279).

Discussion

The Avebury area exhibits a high potential for the study of secondary defensive networks and communications in Anglo-Saxon England. The close proximity of the Ridgeway, which is termed a *herepath* in a charter of 939 relating to the estate of East Overton (Sawyer, 1968: no. 449; Eagles, 1986: 110), is important as the ancient trackway appears to form the spine of the early road network of the region.

Further sections of *herepath* routes are to be considered in future research; a short track, running north-south between the Devizes road and the Bath road west of Beckhampton is also called a *herepath*, although its full course is unclear. It is possible that the route begins at Horton, some 8 km south of Yatesbury, where a Herepath House and Herepath Farm are situated adjacent to a track which runs northward up to the Devizes road at Beckhampton. Work to date has indicated that it will be possible to reconstruct much of the Anglo-Saxon road network in the northern part of the county and beyond.

The theme of this note is to be fully developed in a future paper.

PUBLICATION OF RESULTS

In order that results may be available as soon as possible, reports are written for each discrete unit of work undertaken and submitted to relevant journals for publication. It is intended that interim reports will appear in the *Institute of Archaeology Bulletin* and in the yearly excavation and fieldwork listings in *Medieval Archaeology* and other periodicals.

Significant artefacts or aspects of larger programmes of work may be published separately, for example, the six Neolithic axes from Freeth Farm (Charlton, forthcoming).

Papers of synthesis will be published once it is felt that an area of research has reached a point where general patterns or trends have been identified (e.g. environmental survey) or when a study with a definable limit has been completed (e.g. fieldwalking).

DISCUSSION

It is the purpose of research not only to collect and interpret data but to develop and enhance the methodologies involved in its collection. The problems of site formation processes are being approached by three-dimensional plotting of artefacts and ecofacts. Particular emphasis

is placed on recording interfaces between contexts in addition to the more usual descriptions of deposits themselves. Recording systems are to be developed where needs become apparent.

It is clear that the techniques of landscape archaeology need to be complemented by the excavation of selected sites to provide a chronological framework. Retrogressive analysis can provide a relative sequence for the division of a landscape and has the potential to reveal primary boundaries but these should be sampled by excavation wherever possible. Too great a reliance is often placed on Ordnance Survey maps as a research tool and fieldwork to date has clearly illustrated the hazards of such approaches to settlement studies. The results of excavation and survey programmes on rural sites have shown time and again just how fluid settlements were in terms of their mobility and internal arrangement (cf. Beresford, 1975; Andrews and Milne, 1979; Wade-Martins, 1980). This situation has certainly been confirmed at Compton Bassett and Yatesbury, although little excavation has so far been undertaken at the former.

CONCLUSION

The Compton Bassett Area Research Project was started just as a number of other large-scale programmes, such as Wharram and Raunds, were drawing to a close. In its broadest sense the aim of the project is to complement the results of these pioneering studies whilst testing their applicability to the landscape of central North Wiltshire.

It will be some time before patterns or trends become apparent but the results so far indicate the complex development of the landscape and its high potential for a wide range of archaeological research.

Abstract

This is the first of a series of interim reports concerning the results of fieldwork and desk-based research undertaken on behalf of the Compton Bassett Area Research Project. Interim reports are seen as a vehicle for the discussion of current hypotheses regarding the study area and other related research areas that become identified as the work of the project progresses. Any comments about the research programme or requests for more detailed results would be welcomed.

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Book Reviews

DOUMET, Claude. *Sceaux et cylindres orientaux: la collection Chiha*, Orbis Biblicus et Orientalis, Series Archaeologica 9, Fribourg/Göttingen, 1992. 210 pp.; 24 pls; ISBN 3-7278-0816-0.

This is a catalogue of a private collection comprising 400 stamp and cylinder seals purchased by Michel Chiha in Lebanon between 1925 and 1945. It was written by Madame Doumet under the supervision of P. Amiet in partial fulfillment of a degree at the Ecole du Louvre. Spanning three millennia from the Uruk/Jemdat Nasr to the Sassanian period, the seals are organized by origin, date and style on the basis of comparisons taken in part from controlled excavations published up to 1989, when this work was submitted for publication.

The catalogue is introduced by a history of the main glyptic phases covered by the collection. In reviewing the background of the conventional terminology, Madame Doumet considers the revisions proposed in recent studies before selecting the scheme which best suits her material. In keeping with current consensus, she retains the term 'Jemdat Nasr' despite its shortcomings when applied outside southern Mesopotamia. Frankfort's original tripartition of the Early Dynastic Period is favoured over subsequent adjustments reflecting regional developments, presumably because all seals in this collection are unprovenanced. For the last centuries of the third millennium BC, the author adheres to the sequence established by R.M. Boehmer, although this applies mainly to combat scenes and does not reflect contemporary developments in Syria and southeast Iran, about which little is known. The Syrian glyptic of the early second millennium BC is divided into two groups according to P. Amiet and D. Collon: the one coincides with the later Colony Period and the reign of Zimrilim of Mari; the other spans the Kingdom of Aleppo and its destruction before the rise of Mittani. Although nowhere stated, it is apparent from the dates enclosed in parentheses that the author has adopted the middle chronology. The conventional dichotomy of Mittanian glyptic into 'Common Style' and 'Elaborate Style' is used despite its acknowledged deficiencies, as these can only be resolved by future discoveries. Syrian/ Levantine seals of the later second millennium BC, marked by their diverse subject matter and the lack of reliable dates, are grouped into three iconographic categories following B. Teissier. No distinction is made between Neo-Assyrian and Neo-Babylonian styles.

The main catalogue is divided into three parts: A. Mesopotamian glyptic, B. the seals of Syria (including Mittanian), Anatolia and Cyprus, and C. Forgeries. Each part commences with an historical introduction followed by the entries of stamp seals and cylinder seals arranged in chronological order. In like fashion, the forgeries are arranged according to the style that they imitate. The description in each entry includes reference to seal type, material, dimensions, state of preservation and comparisons where applicable. Each is accompanied by a line drawing

(1:1), whose accuracy can be confirmed in the excellent photographic plates at the end.

There is little fault to be found with this compact and well-researched presentation of an eclectic collection of unprovenanced seals, and the following remarks concentrate on the contributions of recent research into early stamp seals, presentation scenes, Mittanian 'Common Style', Kassite, Middle Assyrian, Neo-Assyrian and Neo-Babylonian glyptic, published since 1989.

The classification of prehistoric stamp seals has been amended and updated in a recent publication by A. von Wickede (*Prähistorische Stempelglyptik in Vorderasien*, Münchener Vorderasiatische Studien VI, Munich 1990). This study, based on newly excavated material, re-confirms Doumet's statement that the stamp seal originates in northern Syria during the aceramic Neolithic (late PPNB, seventh millennium BC)—well before its appearance in northern Mesopotamia during the Halaf period (cf. pp. 14, 103). The typology of the stamp seal is determined first by its back rather than by its engraved face. Nos. 1–8 belong to the solid, handleless category; no. 9, to the handled variety with base and projecting handle. Figure representations are encountered as early as the Neolithic period, with the drilled style beginning as early as Obed 3 (preceding Gawra XIII, cf. p. 103).

In a survey of the 'Presentation scene' M. Haussperger identifies certain guidelines for dating this genre within the Ur III dynasty. According to her list, no. 101 dates from the reign of Amar-Suen on account of the crescent moon, the worshipper's robe and the deity's throne (*Die Einführungsszene*, Münchener Vorderasiatische Studien XI, Munich 1991, pp. 257–8).

Research of dated Middle Assyrian scenes has shown that single horizontal animals beside a tree are typical of Shalmanesar I's reign in the thirteenth century BC (D. Matthews, *Principles of Composition in Near Eastern Glyptic in the Later Second Millennium B.C.*, Orbis Biblicus et Orientalis, Series Archaeologica 8, Fribourg, Switzerland 1990, p. 95). While Doumet traces the goat on no. 130 to a fourteenth-century BC example from the archive Assur 14446, it should be noted that this archive is not, as Beran believed, confined to this century.

The reigning confusion between Neo-Assyrian and Neo-Babylonian designs has been much relieved by two recent investigations. That on Neo-Assyrian glyptic places our dating of the northern styles on a sounder footing by focusing on inscribed and dated impressions of cylinder and stamp seals mainly from Nineveh and Nimrud (Suzanne Herbordt, *Neu Assyrische Glyptik des 8.-7. Jh. v. Chr.*, State Archives of Assyria I, 1992). Whereas the Assyrian 'linear style' does not occur on seventh-century BC tablets, the sintered quartz version (nos. 139, 141–4, 147–9), which Doumet includes in this category, is attested together with the similarly long-lasting 'drilled style'. The 'cut style', which Doumet associates with eighth-century BC Babylonia (p. 75), has Assyrian precursors of ninth-century BC date based on recently published evidence from Hasanlu IVB.

Alalakh levels VI and V, once dated to the seventeenth century BC (cf. p. 145), is now correlated with the Middle Bronze IIC/Late Bronze I period dated *c.* 1575–1460 BC. Nuzi stratum II, now seen to be younger than Alalakh IV, coincides with the fourteenth rather than the fifteenth century BC.

The inclusion of probable forgeries is commendable, as it enables their authenticity to be re-evaluated in light of new

discoveries. An explanation of the reasons for their identification as fakes would have been edifying as well as entertaining, especially for the layman, who will also find this catalogue of great use.

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DAVID R. HARRIS (ed.) *The Archaeology of V. Gordon Childe*. London: UCL Press, 1994. xii; 148 pp.; ISBN 1-85728-220-5. £30.00.

The personality of Gordon Childe was a puzzle to his friends and the character of his work was an enigma to his colleagues. Both groups seriously underestimated his achievement, with the result that his death in 1957 left them with a problem. One group asked themselves how this striking eccentric could possibly have taken his own life, and many refused to believe it. The other group asked themselves how seriously they were to take his references to Marxist philosophy, and for the most part they concluded that these were among his idiosyncracies, a kind of harmless coat-trailing to provoke the archaeological establishment. Too late in the day both were to learn the true seriousness of his intentions. There was the belated publication of a letter in which he explained his reasons for suicide, and there was a gradual appreciation of the range and quality of his writings on the social and political issues of the day. It seems very doubtful whether many of the archaeologists who were close to Childe really knew him at all.

To a considerable extent that is because he concealed his opinions from those who might have rejected them, and, in turn, his contemporaries were quite content to overlook his theoretical writings and to treat him as a more or less respectable British empiricist. By praising his work for entirely the wrong reasons, they must have increased his isolation. It is only with hindsight that we may be able to see his work in perspective.

The only way of considering his life and work is by treating them together, for that work is the only clue to his real motivation. It was his social and political commitments that seem to have provided the mainspring for his work, and they allow occasional glimpses of the man behind his masks. But here again there are questions, for Childe was remarkable in still another way. If he was an enigma in life, his legacy raises problems, and his heirs in prehistoric archaeology are still wrangling over the will. Most would accept that Gordon Childe was the most significant archaeological theorist of the century, but it is difficult to establish his real place in the history of the discipline. He approached his archaeology with a moral seriousness that probably makes him unique and his research grew out of an engagement with the world in which he lived. His very integrity has added to his stature, but it also means that every school wants him as an intellectual ancestor.

The Childe centennial conference considered those various claims and by treating his theoretical achievement as seriously as his other writings it treated him seriously too. Although little of the material presented at this meeting was anecdotal, the resulting publication comes close to providing a portrait of this remarkable man. But the picture that results is a kind of cubist portrait in which the same figure is seen simultaneously from a myriad of different viewpoints. Colin Renfrew sees Childe as a forerunner of processual archaeology, but for Bruce Trigger he anticipates the ideas of the postprocessual school. In their different ways

Mike Rowlands and Leo Klejn explore his relations with Marxist archaeology, and Kent Flannery examines his influence on the archaeology of the New World. No one comes away empty handed. In life Childe had several personae and he wrote in at least two genres, the empirical and the theoretical. Such a massive *oeuvre* has something for everyone. His ideas foreshadow many of the more interesting developments of the last quarter century.

What comes out of this volume is not just the range of his imagination and the depth of his knowledge. If Childe's work was ahead of its time, it was also its product. Several of the papers explore the profound influence that the events of his own day had on his views of the past. John Mulvaney shows very clearly how actively Childe had engaged in Australian politics and how he was made to suffer for his beliefs. Kent Flannery's contribution suggests that it was his pacifism that made him so reluctant to talk about warfare in prehistory, and Mike Rowlands' chapter, one of the best in the book, talks most convincingly about Childe 'the poser of questions about human freedom and responsibility to be answered by archaeology'.

It is a measure of the value of this book that despite their different perspectives all the papers treat Childe's theoretical work in depth. That applies as much to the more philosophical contributions as it does to Leo Klejn's extraordinary and sensitive account of Childe's difficult relationship with Soviet archaeology. It also extends to the transcript of the discussions that enlivened the meeting in London. Many of the questions raised quite fundamental issues, and a number of the answers, particularly Bruce Trigger's, were thoughtful, detailed and carefully considered. There was none of the empty rhetoric that usually takes place on such occasions.

It was in those discussions, as well as in the papers, that we were allowed to glimpse Childe the human being, a man who was socially inept and intellectually isolated, secure in his own opinions but unwilling to upset others by voicing them when they were unwelcome. This book allows us to approach his complex personality through accepting the complexity of his work. In that respect it makes amends for the short-sightedness of so many of his closest colleagues.

The conference was remarkable because all the papers had something important to say, and the publication catches the spirit of that meeting. In concert with the Prehistoric Society, the Institute of Archaeology has done Childe justice by inspiring a thoroughly worthwhile volume.

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PELTENBURG, Edgar. A Ceremonial Area at Kissonerga. *Lemba Archaeological Project volume II.2*. S.I.M.A. Vol. LXX:3. Göteborg: Paul Åström Förlag, 1991. 117 pp.; 18 pls (2 colour); 51 figs, ISBN 91-7081-0117. Pbk.

This publication records the findings from the fifth and sixth seasons of the Lemba Archaeological Project's new excavations at Kissonerga-*Mosphilia*, a multi-period site located in the Paphos region of south-west Cyprus. More precisely it concentrates on a number of Chalcolithic features and deposits which, according to Peltenburg, can be characterized as comprising a 'ceremonial area'. This represents an attempt to offer a radical, new ideological perspective in Cypriot studies as far as ritual behaviour is concerned.

The introduction and first two chapters concentrate on the location and evolutionary history of the structures in the ceremonial area, and in particular two Middle Chalcolithic features. These comprise units 1015 and 1225, located beneath the floor of a Late Chalcolithic structure (994), which contained a unique cache, a deliberate deposit in a non-mortuary context. The term 'unique' does seem germane since the combined configuration and content of these pits appear unparalleled in Cyprus at this time (the deposit has been dated to *c.* 3000 BC). Descriptions of the ceremonial area as a whole are extremely detailed and thorough although I would question whether sometimes description and interpretation become indistinguishable, with perhaps an inability to obtain complete objectivity. Something which is not discussed is speculation about whether access to this area may have been controlled through the location of surrounding structures and their doorways (i.e. the area had restricted access).

It is not surprising that the most highlighted feature is the 'enigmatic' circular building Model KM1446 (now in the Nicosia Museum, Cyprus), claimed to replicate actual structures uncovered at Kissonerga-*Mosphilia*. However, I apply a note of caution. The model should not lead to a sense of complacency regarding our assumptions of what contemporary houses were actually like. Peltenburg does acknowledge this premise in his introduction making it clear that any reconstructions are tentative due to the uniqueness of unit 1015 in particular.

Chapter Two discusses the ways in which the model mirrors and differs from what we know of contemporary buildings and goes on to speculate concerning the possible functions of the model in conjunction with comparative evidence from other building models, especially the Vounous model (all of which are clearly illustrated). Suggested roles for such models in prehistory include funerary and cult deposits, playthings and foundation offerings. However, as Peltenburg notes, the use of cross-cultural comparisons is perhaps of limited value and merely highlights the diverse nature of building models and their associated contexts (p. 26). His contextual 'reading' of the archaeology perhaps records his unease concerning the dilemma of placing the building model in its correct place within society. In the case of Model KM1446 and the ceremonial area, the connection with fertility, rites of passage, and the 'awesome event' of childbirth are recurrent themes. I would argue that some of the connections are clutching at straws, for example, the suggestion that the intact Charonia shell found within the model may have been used to pour cold water over hot stones to produce steam (as an accompaniment to childbirth) is perhaps presupposing both functional and rational behaviour. One of the most interesting facts about the model (pp. 99–100) is what is not included, i.e. no functional tools (chisels, axes, adzes) or personal items (pendants), and more surprisingly, no picrolite figurines which have strong associations with birth and fertility (see Peltenburg 1991 BASOR 282/283 pp. 107–26).

Separate chapters are given over to detailed descriptions and interpretations of the contents of these pits, other ceramics, figurines, the groundstone artefacts, lithics, animal and molluscan remains. One hopes that repetition of these are avoided in the awaited final report. However, I appreciate the need to isolate the description of the ceremonial area should it become lost in the final publication. The illustrations and photographs are faultless and it is worth picking up the

volume merely to flick through the pictures. Curiosity will then probably result in further reading. The publication sets a standard to which others working in Cyprus should aspire.

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Notes to Contributors

Papers on any aspect of archaeology may be considered for publication in the *Bulletin*. All contributions and related correspondence should be addressed to the Editor, Professor J. J. Wilkes, Institute of Archaeology, 31–34 Gordon Square, London WC1H 0PY. The following notes are provided as a guide to intending contributors in the preparation of their material.

1) *Typescripts* should not normally exceed 10,000 words in length. They should be typed on one side only of A4 size paper (approx. 30 cm by 21 cm), using double spacing and leaving wide margins (at least 4 cm on the left). Two copies of the typescript should be submitted, the author retaining a third copy (complete with duplicate figures, plates and tables). If possible, authors are asked to provide, in addition, a copy of the text on disk (preferably in Microsoft Word for Macintosh or ASCII files), specifying the format.

2) *Footnotes* should be avoided as far as possible. If any are judged to be absolutely necessary, they should be typed on a separate sheet, not at the foot of the page to which they refer.

3) *Bibliographical references* should follow the so-called Harvard system. The author's last name, date of publication and the significant page number should be given in brackets in the body of the text, e.g. (Sharma, 1973: 29), or, if the author's name has been cited, simply (1973: 129). Full references should be listed alphabetically according to the authors' names at the end of the paper. For example:

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1973 Mesolithic lake cultures in the Ganga valley, India, *PPS* 39: 129–46.

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1972 *Environment and Archaeology* (London: Methuen).

for an **article in a book:**

Bordes, F.
1973 On the chronology and contemporaneity of different palaeolithic cultures in France, in *The Explanation of Cultural Change: Models in Prehistory,* C. Renfrew ed., (London: Duckworth), 146–200.

Where the publication cited is a paper in a periodical the title of the journal should be italicized (or underlined) and abbreviated using, if possible, the preferred abbreviations given in the *List of Abbreviations of the Archaeological Literature* published in *Acta Praehistorica et Archaeologica,* 9/10, 1978/9, 271–383. If this is not accessible, there are various other lists which may be consulted, e.g. the ones printed from time to time in the *American Journal of Archaeology,* the *Index des Périodiques* supplement to *l'Année Philologique*, Vol. 51, 1982 (for

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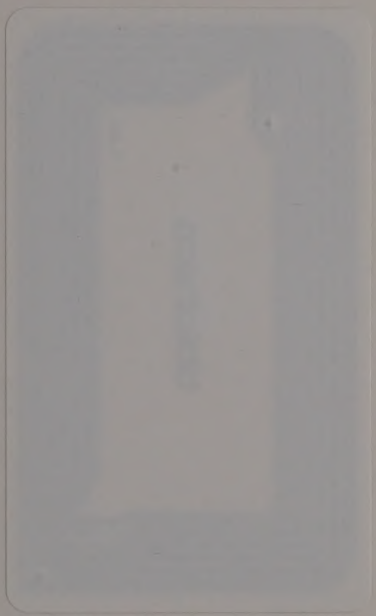
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